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OF THE

ROYAL SOCIETY OF LONDON.

From Feb. 23, 1854 *to Dec.* 20, 1855 *inclusive*

(BEING A CONTINUATION OF THE SERIES ENTITLED "ABSTRACTS OF THE PAPERS COMMUNICATED TO THE ROYAL SOCIETY OF LONDON").

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VOL. VII.

February 23, 1854.

The Rev. BADEN POWELL, V.P., in the Chair.

The following communications were read:—

I. A paper entitled, "Continuation of the subject of a paper read Dec. 22, 1853, the supplement to which was read Jan. 12, 1854, by Sir FREDERICK POLLOCK, &c.; with a proof of Fermat's first and second Theorems of the Polygonal Numbers, viz. that every odd number is composed of four square numbers or less, and of three triangular numbers or less." By Sir FREDERICK POLLOCK, M.A., F.R.S. &c. Received February 23, 1854.

The object of this paper is in the first instance to prove the truth of a theorem stated in the supplement to a former paper, viz. "that every odd number can be divided into four squares (zero being considered an even square) the algebraic sum of whose roots (in some form or other) will equal 1, 3, 5, 7, &c. up to the greatest possible sum of the roots." The paper also contains a proof, that if every odd number $2n+1$ can be divided into four square numbers, the algebraic sum of whose roots is equal to 1, then any number n is composed of not exceeding three triangular numbers.

The general statement of the method of proof may be made thus: two theorems are introduced which connect every odd number with the gradation series, 1, 3, 7, 13, &c., of which the general term is $n+n^2+1$ or $4p^2 \pm 2p+1$ (that is, the double of a triangular number $+1$), each term of which series can be resolved into four squares, the algebraic sum of the roots of which, $p, p, p, p+1$, or $p-1, p, p, p$ may manifestly be $=1$. By these theorems it is shown that every odd number is divisible into four squares, having roots capable of

forming as the sum of the roots 1, 3, 5, 7, &c. up to the greatest possible sum of the roots.

As the four square numbers which compose an odd number must obviously be three of them even and one odd, or three odd and one even, the differences of the roots among themselves must be the first odd and the third even, or *vice versâ*; and therefore these roots must have the sum of the first and third differences an odd number; the middle difference may be either odd or even.

The first of the theorems referred to, called by the author "Theorem P," is in substance this:—

Let r, s, t, v be the roots the squares of which compose any odd number N, such that $r+s+t+v=1$, and let each of these roots be increased by m; then $r+m$, $s+m$, $t+m$, $v+m$ will be the roots of the odd number $N+2m(2m+1)$; and $m-r$, $m-s$, $m-t$, $m-v$ the roots of the odd number $N+2m(2m-1)$; the sum of the roots in the first case being $4m+1$, and in the second $4m-1$. So that giving to m successively the values 0, 1, 2, 3, &c. in the general form $N+2m(2m\pm1)$, a series will be formed in which the sums of the roots will be 1, 3, 5, 7, 9, &c., and the sums of their squares N, $N+2.1.1$, $N+2.1.3$, $N+2.2.3$, $N+2.2.5$, $N+2.3.5$, $N+2.3.7$, $N+2.4.7$, &c.; or N, $N+1.2$, $N+2.3$, $N+3.4$, $N+4.5$, $N+5.6$, $N+6.7$, $N+7.8$, &c. So that if p be the distance of any odd number in this series from N, the number will be $N+p(p+1)$, and the sum of its roots will be $2p+1$.

The conclusions to be drawn from this theorem are then stated:—

1. The greatest sum of the roots of the squares into which any odd number can be divided may be obtained: for let $2n+1$ be any odd number, and $2p+1$ the odd number to which the algebraic sum of its roots is required to be equal; then if p is such that $p(p+1)$ is less than $2n+1$, the number $2n+1$ can be resolved into squares the sum of whose roots is $2p+1$; otherwise it cannot.

2. The form of the roots of $2n+1$ may be found of which the algebraic sum is any possible odd number $2p+1$ except 1, provided all the odd numbers less than $2n+1$ possess the property of having the algebraic sum of their roots $=1$. For if from $2n+1$, $p(p+1)$ be taken, there will remain an odd number (N in Theorem P) such that, according to the condition stated, the algebraic sum of its roots $=1$; and in the series of roots and odd numbers formed from

these roots according to theorem P, p terms from N will be found the number $2n+1$ composed of squares the algebraic sum of whose roots is $2p+1$.

It thus appears that any odd number $2n+1$ can be divided into squares the sum of whose roots will equal 3, 5, 7, &c. (any possible odd number except 1) if the odd numbers below it can be divided into squares the sum of whose roots $=1$; and if it can be shown that its roots in some form will equal 1, then the theorem M will be true for that number and for every number below it.

This is illustrated by an example, and then another theorem, called "Theorem Q," is stated. In this a series of roots and odd numbers is formed by making the 1st and 3rd differences of the roots constant, but reversed every alternate term, and increasing or diminishing the middle difference by 1 each term;—or the middle difference is made constant and the 1st and 3rd vary. The sums of the roots thus become constant in every term of the series, but the sums of the squares of the roots increase, as in theorem P, by the even numbers 2, 4, 6, 8, &c., so that the increase at any number of terms p is $p(p+1)$, or the double of a triangular number.

By the application of these theorems to a variety of examples, it is shown how any odd number may be composed of four squares, such that the algebraic sum of their roots may equal 1.

The theorems P and Q, it is considered, connect every odd number with every other odd number, so as to make it impossible if one odd number be composed of four squares, but that every other odd number should likewise be so. It is pointed out in what manner every possible combination of numbers which can furnish the differences of the roots of any squares, not exceeding four, which can make an odd number, and the sum of which roots $=1$, can be derived from the gradation series, that is from $4p^2\pm 2p+1$. The combined effect of the theorems P and Q is therefore to prove that every odd number must be composed of not exceeding four square numbers.

The author goes on to show that every number is composed of not exceeding three triangular numbers, by proving that if every odd number $2n+1$ can be divided into four square numbers the sum of whose roots $=1$, then n will be composed of not exceeding three triangular numbers. This is done by taking the differences of the

roots of $2n+1$, the algebraic sum of which roots is one, and diminishing the middle difference by theorem Q until it reaches a number nearest to half the sum of the first and third differences. The difference between $2n+1$ and the number thus obtained will be the double of a triangular number $=2T$. By the next step, the extreme differences are reduced until they are of the form m, $m+1$; and the difference between $2n+1-2T$ and the number thus obtained will again be the double of a triangular number $=2T^1$. The differences last obtained give the double of a triangular number $+1=2T''+1$. So that we find $2n+1=2T+2T'+2T''+1$. Consequently $n=$ the sum of three triangular numbers, if all the three operations be necessary; if not, to two or one triangular number only.

II. The first part of a paper "On a Class of Differential Equations, including those which occur in Dynamical Problems." By W. F. Donkin, M.A., F.R.S., F.R.A.S., Savilian Professor of Astronomy in the University of Oxford.

This paper is intended to contain a discussion of some properties of a class of simultaneous differential equations of the first order, including as a particular case the form (which again includes the dynamical equations),

$$x'_i=\frac{dZ}{dy_i},\quad y'_i=-\frac{dZ}{dx_i}, \qquad \text{(I)}$$

where $x_1 \dots x_n$, $y_1 \dots y_n$ are two sets of n variables each, and accents denote total differentiation with respect to the independent variable t; Z being any function of x_1 &c., y_1 &c., which may also contain t explicitly. The part now laid before the Society is limited to the consideration of the above form.

After deducing from known properties of functional determinants a general theorem to be used afterwards, the author establishes the following propositions.

If $x_1 \dots x_n$ be n variables connected with n other variables $y_1 \dots y_n$ by n equations of the form $y_i=\frac{dX}{dx_i}$ (X being a given function of $x_1 \dots x_n$); then the equations obtained by solving these algebrai-

cally, so as to express $x_1 \ldots x_n$ in terms of $y_1 \ldots y_n$, will also be of the form $x_i = \frac{dY}{dy_i}$; where Y is a function of $y_1 \ldots y_n$, which may be defined by the equation

$$Y = -(X) + (x_1)y_1 + \ldots + (x_n)y_n,$$

in which the brackets indicate that the terms within them are to be expressed as functions of $y_1 \ldots y_n$. Moreover, if p be any other quantity contained explicitly in X (besides the variables $x_1 \ldots x_n$), the following relation will subsist; namely,

$$\frac{dX}{dp} + \frac{dY}{dp} = 0,$$

the differentiation in each case being performed only so far as p appears *explicitly* in the function.

It is then shown that if X contain explicitly, besides $x_1 \ldots x_n$, the n constants $a_1, a_2, \ldots a_n$, and the variable t, and if the $2n$ variables $x_1 \ldots x_n, y_1 \ldots y_n$, be determined as functions of t by the system of $2n$ equations,

$$\frac{dX}{dx_i} = y_i, \quad \frac{dX}{da_i} = b_i, \quad \ldots \ldots \ldots \quad \text{(II.)}$$

where $b_1 \ldots b_n$ are n other constants, the elimination of the $2n$ constants from these equations and their differentials with respect to t, leads to the system of differential equations (I.), if for Z be put the result of substituting in $-\frac{dX}{dt}$ the values of the $2n$ constants in terms of the variables. The equations expressing the $2n$ constants in terms of the variables may be considered as the $2n$ integrals of the system (I.).

The author employs the symbol $[p, q]$ in a sense similar to that in which Poisson and others have employed (p, q), namely, as an abbreviation for $\Sigma_i\left(\frac{dp}{dy_i}\frac{dq}{dx_i} - \frac{dp}{dx_i}\frac{dq}{dy_i}\right)$; and he shows that if p, q represent any two of the $2n$ constants a_1 &c., b_1 &c., then $[p, q]$ is either $=1$ or $=0$, according as p, q are a *conjugate* pair a_i, b_i, or not.

Next it is shown that if $a_1, a_2, \ldots a_n$ represent any functions of $2nn$ variables $x_1 \ldots x_n, y_1 \ldots y_n$, satisfying identically the $\frac{n(n-1)}{2}$ conditions $[a_i, a_j] = 0$, then if by means of the n given equations ex-

pressing a_1, &c. in terms of the variables, the set $y_1, y_2 \ldots y_n$ be expressed as functions of $x_1 \ldots x_n$, $a_1 \ldots a_n$, the $\frac{n(n-1)}{2}$ relations $\frac{dy_i}{dx_j} = \frac{dy_j}{dx_i}$ will be identically satisfied; in other words, the expression for $y_1 \ldots y_n$ will be the partial differential coefficients of a function of $x_1 \ldots x_n$.

Hence it easily follows, that if any n integrals $a_1 \ldots a_n$ of the system (I.) be given, which satisfy the conditions $[a_i, a_j]=0$, a "Principal Function" X can always be found, from which the remaining integrals of the system may be deduced by means of the second set of equations (II.).

The relation in which these investigations stand to the discovery of Sir W. R. Hamilton (as improved and completed by Jacobi) is pointed out. And it is shown that the system of n differential equations of the second order

$$\left(\frac{d\mathrm{W}}{dx'_i}\right)' = \frac{d\mathrm{W}}{dx_i}$$

(to which Lagrange had reduced the dynamical equations, and which Sir W. Hamilton had transformed into the system (I.) by a process depending upon the circumstance that, in dynamical problems, W contains $x'_1, x'_2, \ldots x'_n$ only in the form of a homogeneous function) may, by means of the theorems established at the beginning of the paper, be reduced to the form (I.) without assuming anything as to the form of W, which may be any function whatever of $x_1 \ldots x_n$, $x'_1 \ldots x'_n$, and t.

The $2n$ integrals of the system (I.), obtained in the way above explained, being shown to satisfy the conditions

$$[a_i, b_i]=1, \quad [a_i, a_j]=[a_i, b_j]=[b_i, b_j]=0,$$

it is proposed to call them "normal integrals," and the constants a_1 &c., b_1 &c. "normal elements," any pair a_i, b_i being called *conjugate*.

In the second section, the author gives a simplified demonstration of Poisson's theorem (extended to the general system (I.)), that if f, g be any two integrals, $[f, g]$ is constant. The preceding principles are then exemplified by application to the problems of the motion of a material point under the action of a central force, and the rotation of a solid body about a fixed point.

In each case three integrals, c_1, c_2, c_3, are taken, satisfying the three conditions $[c_2, c_3]=0$, $[c_3, c_1]=0$, $[c_1, c_2]=0$; the first being the integral of *vis viva*, and the other two being derived from the integrals expressing the conservation of areas. In the former problem the "principal function" is then found with great ease, and the remaining integrals deduced. The set of "normal elements" thus obtained coincide with those given by Jacobi (in a memoir in Crelle's Journal, vol. xvii.). In the problem of rotation, the algebraical solution of the three assumed integrals for y_1, y_2, y_3 depends upon that of an equation of the fourth degree. It is therefore impracticable to exhibit the principal function in an explicit form. In this respect the result arrived at resembles that obtained by Mr. Cayley in a totally different way; Mr. Cayley having shown that the solution of the problem is reducible to quadratures, assuming the algebraical solution of a certain system of equations of the same form as those to which the author of the present investigation is conducted. (Camb. and Dub. Math. Journ. vol. i. p. 172.)

Methods are then indicated by which, when one system of "normal elements" is given, other systems may be found.

The practical value of "normal solutions" of the system (I.) depends chiefly upon the simplicity of the corresponding formulæ for the variation of elements, the theory of which is intended to form part of the subject of the following sections.

March 2, 1854.

Professor GRAHAM, V.P., in the Chair.

In accordance with the Statutes, the Secretary read the following list of Candidates for election into the Society.

James Allman, M.D.
Henry Foster Baxter, Esq.
Edward William Brayley, Esq.
Alexander Bryson, M.D.
James Caird, Esq.
J. Lockhart Clarke, Esq.
William Coulson, Esq.
Thomas Russell Crampton, Esq.
Joseph Dickinson, M.D.
Solomon Moses Drach, Esq.
Major Duckett.
John Eric Erichsen, Esq.
Sir Charles Fox.
Ronald Campbell Gunn, Esq.
William Bird Herapath, M.D.
Robert Hunt Esq.
John Bennet Lawes, Esq.
Edward Joseph Lowe, Esq.
Robert Mallet, Esq.
Charles May, Esq.
Captain Moore, R.N.
Henry Perigal, Esq.
Captain Strachey.
R. D. Thomson, Esq.
Charles Vincent Walker, Esq.
Samuel Charles Whitbread, Esq.
Robert Wight, M.D.
Thomas Williams, M.D.
W. C. Williamson, Esq.
George Fergusson Wilson, Esq.

The following Papers were read :—

I. "On the Growth of Land Shells." By E. J. Lowe, Esq., F.G.S., F.R.A.S. &c. Communicated by Henry Lawson, Esq., F.R.S. Received February 18, 1854.

Perhaps the following observations on the growth of land shells may contain sufficient information to prove interesting to the Royal Society. Before describing them, however, a few introductory remarks will be necessary. Every individual experimented upon has been kept in confinement since the day it was hatched. Each

species has been placed in a separate box (filled with soil to the depth of three inches), and care has been taken to feed the Mollusca every other day, the food chiefly consisting of the leaves of the lettuce and cabbage. In very dry weather the soil has been moistened with rain-water about once a week; in that box containing *Helix pomatia* small lumps of chalk have been mixed with the soil.

The species experimented upon were:—

Helix aspersa	Zonites cellarius
— caperata	— lucidus
— hispida	— nitidulus
— nemoralis	— radiatulus
— pomatia	Bulimus obscurus
— rotundata	Clausilia nigricans
— virgata	Pupa umbilicata

The facts arrived at are,—

1st. The shells of Helicidæ increase but little for a considerable period, never arriving at maturity before the animal has *once* become dormant.

2nd. Shells do not grow whilst the animal itself remains dormant.

3rd. The growth of shells is very rapid when it does take place.

4th. Most species bury themselves in the ground to increase the dimensions of their shells.

First Experiment with Helix pomatia.

A specimen of this species having deposited thirteen eggs which were hatched during the first week of August 1852, six of the young ones were deposited in a box (having a lace cover) placed in the shade. The young Helices were regularly fed every other day until the beginning of December, when they buried themselves in the soil for winter; up to this period they had gradually increased in dimensions to the size of *Helix hispida.* From December until April the soil was kept dry, the box being placed in the cellar. On the 1st of April they were replaced in the garden, the soil having previously been copiously watered. On the 3rd of April the young ones appeared on the surface, being *no larger* in size than they were in December, and although regularly fed up to the 20th of June

they scarcely increased, not being perceptibly larger in size than they were in December. However, on the 20th of June five of them disappeared, having buried themselves (with the mouth of the shell *downwards*) in the soil; on the 30th of June they reappeared, having in ten days grown so rapidly as at this time to become equal in size to *Helix pisana*. They again buried themselves on the 15th of July and reappeared on the 1st of August, having again increased in size. From this date they did not apparently become any larger, and on the 2nd of November food was withheld for the winter, and at the present time (February 14th) they are in a dormant state. Probably they will arrive at maturity by July or August next. The sixth specimen did not bury itself until the 15th of August.

Second Experiment with Helix aspersa.

A pair of *Helix aspersa* having been procured in the act of copulation on the 19th of May 1852, they were placed in confinement. Each individual deposited about 70 eggs, which began to hatch on the 20th of June: these young ones grew but little during the summer. They buried themselves in the soil on the 10th of October, coming again to the surface on the 5th of April, *not having grown during the winter*. In May they buried themselves (with their *heads downwards* as with *Helix pomatia*, in winter they and other species buried themselves with the *head upwards*), appearing again in a week *double the size;* this process was carried on at about fortnightly intervals until July the 18th, when they were almost fully grown. It is worthy of remark that this species, as well as *Helix pomatia* and *Helix nemoralis*, and probably other of the Helicæ, form an operculum at the aperture, after which they retire considerably within the shell, and form a second (much thinner), behind which they rest during the winter.

It would be swelling this paper too much to describe all the observations in full; it will perhaps therefore be considered sufficient to remark that the process of growth *within the ground* takes place with *Helix nemoralis, Helix virgata, Helix caperata*, and *Helix hispida*. *Helix rotundata* burrows into decayed wood to increase the size of its shell. *Zonites radiatulus* appears to remain on decaying blades of grass; whilst *Pupa umbilicata, Clausilia nigricans* and *Bulimus obscurus* bury *their heads only* to increase their shells. With respect

to *Zonites cellarius*, *Zonites lucidus*, and *Zonites nitidulus*, it was not satisfactorily ascertained whether their heads were buried during the process of growth.

E. J. Lowe.

Observatory, Beeston,
1854, February 14th.

II. "Note on the Decomposition of Sulphuric Acid by Pentachloride of Phosphorus." By Alexander Williamson, Ph.D., F.C.S., Professor of Practical Chemistry in University College. Communicated by Dr. Sharpey, Sec. R.S. Received February 23, 1854.

Chemists have long been aware of the fact that some acids unite with bases in one proportion only, others in two or more proportions. Thus a given quantity of nitric acid forms with what is termed its equivalent of potash, a definite nitrate of potash; if less than this equivalent quantity of potash were added to the nitric acid, the product would be a mechanical mixture of the same nitrate of potash with uncombined nitric acid; if more than the equivalent of potash were added, the excess of alkali would remain uncombined. Sulphuric acid, on the other hand, is capable of forming two compounds with potash, and it depends upon the proportions in which the two substances are brought together whether the neutral or acid sulphate is formed.

The number of compounds which an acid forms with one base is now considered as indicating its atomic weight. The weights of sulphuric and nitric acids which are respectively susceptible of neutralizing the same quantity of potash are termed *equivalent*, but these are by no means the same as their *atomic* weights. Sixty-three parts of nitric acid (nitrate of water) contains the same quantity of hydrogen as forty-five parts of sulphuric acid, and when they are neutralized by potash the whole of this hydrogen is removed and replaced by potassium; and if neither of the acids could combine in any other proportion with potash, their atomic weights would be the same as their equivalent weights. But sulphuric acid also forms a potash compound in which half of its hydrogen is replaced by potas-

sium, the other half remaining in the compound, whereas the smallest particles of nitric acid either exchange the whole or none of their hydrogen for potassium.

This fact is expressed in the simplest possible manner by the statement that the smallest indivisible particles of sulphuric acid contain two atoms of hydrogen, whilst those of nitric acid only contain one. Thus it is, that whereas the equivalent weights of the two acids are the quantities which contain the same amount of basic hydrogen, their atomic weights must be in the proportion of two equivalents of sulphuric to one of nitric acid. The simplest expression for an atom of nitric acid being empirically $NO_3\,H$, we shall accordingly represent an atom of sulphuric acid by the formula SO_4H_2. In like manner, an atom of common phosphoric acid, being tribasic, is expressed empirically by the formula $PO_4\,H_3$. The labours of Messrs. Laurent and Gerhardt greatly contributed to the establishment of these results, which are uncontroverted.

We have hitherto been accustomed to resort very freely to imaginary distinctions of form and arrangement of matter to explain the differences of properties; but of late years an opposite tendency has arisen, and chemists have felt the necessity of reducing their language and ideas to simpler and more consistent forms. This necessity was first felt in the most complex, *i. e.* the so-called organic part of chemistry. But the simplifications thus introduced have proved to be equally applicable to the inorganic part of the science; and their introduction is calculated to disengage, for the consideration of substantial differences of composition, the attention which has hitherto been absorbed by imaginary distinctions of form. Being unable to express the constitution of compounds without some formal artifice, we shall be able to see and compare their substantial differences most easily when all unnecessary variations of those formal artifices are eliminated. The success of this operation of course depends on our finding one form sufficiently general to replace the special and limited forms now employed.

In some papers published in the Journal of the Chemical Society two or three years ago, I endeavoured to show that the constitution of salts may be reduced to the type of water; that acids and bases being, truly, acid salts and basic salts, are perfectly conformable to the same principle; and that, amongst other things, the difference

between monobasic and bibasic acids, &c. admits of a simple and easy explanation by it. The leading propositions in those papers have been adopted by several eminent chemists in this country and in France; and M. Gerhardt speedily enriched science with a series of brilliant and striking illustrations of their truth. As regards the constitution of bibasic acids, M. Gerhardt's results were, however, at variance with that theory; and he was led to represent them by formulæ equally inconsistent with his own previous views on the subject. I believe that this discrepancy is satisfactorily removed by the facts I have the honour of submitting to the consideration of the Society.

An atom of nitric acid, being eminently monobasic, is, as we have already shown, represented in the monobasic type $\begin{matrix}H\\H\end{matrix}O$ by the formula $\begin{matrix}(NO_2)\\H\end{matrix}O$, in which peroxide of nitrogen (NO_2) replaces one atom of hydrogen. In like manner, hydrate of potash $\left(\begin{matrix}H\\K\end{matrix}O\right)$ is obtained by replacing one atom of hydrogen in the type by its equivalent of potassium; and nitrate of potash $\left(\begin{matrix}NO_2\\K\end{matrix}O\right)$ by a simultaneous substitution of *one* atom of hydrogen by peroxide of nitrogen, the *other* by potassium. Sulphuric acid is formed from two atoms of water $\begin{matrix}H\\H\end{matrix}O \; \begin{matrix}H\\H\end{matrix}O$; one of hydrogen from each is removed, and the two replaced by the indivisible radical SO_2. The series

Sulphuric acid.	Acid sulphate of potash.	Neutral sulphate of potash.
$\begin{matrix}H\\SO_2\\H\end{matrix}\begin{matrix}O\\O\end{matrix}$,	$\begin{matrix}H\\SO_2\\K\end{matrix}\begin{matrix}O\\O\end{matrix}$,	$\begin{matrix}K\\SO_2\\K\end{matrix}\begin{matrix}O\\O\end{matrix}$

explains itself.

Chemists have long known how to remove the basylous constituents H, K, &c. of these salts, and to replace them by others. But it is only recently that they have learnt to remove the chlorous radicals SO_2, NO_2, &c. in a similar manner. To obtain the chloride of potassium from its sulphate, it is sufficient to bring the latter into liquid contact with chloride of barium; but the same reagent would be powerless for the preparation of the chlorides of the radicals SO_2 or NO_2.

M. Cahours has shown us a reagent (the pentachloride of phosphorus) which is capable of forming from a great number of monobasic acids the chlorides of the acid radicals. Whilst extending our knowledge of the action of the body on monobasic and organic acids, and preparing numerous compounds of their radicals with one atom of chlorine, M. Gerhardt examined also the nature of its action upon bibasic acids and their compounds; and states that it consists of two successive phases, first, the liberation of the anhydrous acid, secondly, the substitution of two atoms of chlorine for one of oxygen in that anhydrous acid. These facts, if correct, would be unfavourable to the above view of the constitution of sulphuric and the other bibasic acids; and M. Gerhardt adopted accordingly the old formulæ, representing in their composition an atom of water ready-formed, $SO_3 H_2 O$.

Confining my remarks for the present to the case of sulphuric acid, whose decomposition is doubtless typical of that of other bibasic acids, I may state as the result of numerous experiments with the most varied proportions of pentachloride and acid, performed on a scale of considerable magnitude, that the first action of the pentachloride consists in removing one atom of hydrogen and one of oxygen (empirically peroxide of hydrogen) from the acid, putting in an atom of chlorine in their place and forming the compound $SO_2 \begin{matrix} H \\ \quad O \\ Cl \end{matrix}$, which is strictly intermediate between the hydrated acid and the final product $SO_2\, Cl_2$ formed by a repetition of the same process of substitution of chlorine for peroxide of hydrogen. The existence and formation of this body, which we may call chloro-hydrated sulphuric acid, furnishes the most direct evidence of the truth of the notion, that the bibasic character of sulphuric acid is owing to the fact of one atom of its radical SO_2 replacing or (to use the customary expression) being equivalent to two atoms of hydrogen. Had this radical been divisible like an equivalent quantity of a monobasic acid, we should have obtained a *mixture*, not a *compound* of the chloride with the hydrate,—or, at least, the products of decomposition of that mixture.

Chloro-hydrated sulphuric acid boils at 145° Cent., distilling without decomposition. The intensity of its action upon water varies according to the manner in which the two bodies are brought

together. When poured rapidly into a large quantity of cold water, a portion of it sinks to the bottom, and only gradually dissolves as a mixture of hydrochloric and sulphuric acids. When a small quantity of water is added to the compound, the same decomposition takes place with explosive violence. The acid dissolves chloride of sodium on the application of a gentle heat with evolution of hydrochloric acid, giving rise to a compound of the formula $SO_2 \begin{matrix} Na\,O \\ Cl \end{matrix}$, When poured upon pieces of melted nitre at the atmospheric temperature, an effervescence takes place with evolution of a colourless vapour which possesses in a striking degree the odour of aqua regia. This vapour may be dissolved in various liquids, and when decomposed by water, yields nitric and hydrochloric acids. It is doubtless chloro-nitric acid, $NO_2\,Cl$. In like manner the chlorides of other inorganic acid radicals may be obtained, as from chlorates, perchlorates, sulphites, &c., but of these and other reactions I beg leave to defer any further account until the experiments now in hand are more advanced.

From the general resemblance of properties and identity of boiling-point of the chloro-hydrate with a compound discovered by Rose, and described by that eminent chemist as possessing empirically the composition $S_2\,O_5\,Cl_2$, I was led to suspect that the two might in reality be identical, which of course would require the addition of the elements of water to Rose's formula, and several experiments I have performed afford strong confirmation of that identity. The same compound is obtained by the action of dry hydrochloric acid on anhydrous sulphuric acid; and finally, I may mention that Mr. Railton obtained a small quantity of the same substance some weeks ago in my laboratory by the action of platinum-black at a high temperature on an imperfectly dried mixture of chlorine and sulphurous acid.

As regards the successive transformations effected in the pentachloride, I have observed the formation of Wurtz's oxychloride (the tribasic chloro-phosphoric acid ($PO\,Cl_3$)), and also of a compound boiling above 145°, probably $PO_2\,Cl$. Hydrated phosphoric acid is always found unless the amount of pentachloride added is very great.

March 9, 1854.

THOMAS BELL, Esq., V.P., in the Chair.

The following paper was read :—

"On a new and more correct method of determining the Angle of Aperture of Microscopic Object-Glasses." By WILLIAM S. GILLETT, Esq., M.A. Communicated by CHARLES BROOKE, Esq., M.A., F.R.S. Received March 9, 1854.

The very large apertures assigned to the more recent microscopic object-glasses drew the author's attention some time since to the importance of testing the accuracy of the method employed to determine their amount.

With this object in view he began with the consideration that the central pencil was alone to be regarded, and that the marginal rays of this were the true limits of the angle of aperture, and that consequently the rays of all oblique pencils were to be excluded, as these might cross at a point not coincident with the principal focus, and being measured separately might form an angle (apparently of aperture) not coinciding of course with the true one, although perhaps not differing from it in amount. A short description of the usual method of measuring these angles will suffice to show what claim it has to confidence in these respects.

The microscope, with the object-glass to be examined and an ordinary eye-piece, is used as a telescope, and a light placed at some distance is commonly made an object to define the limit of the field of view, the image of which is formed near the back surface of the posterior combination, and the diffused light of this image, as seen through the eye-piece, is the indication that a pencil of light is admitted, whether central or oblique. Sometimes by an additional glass the eye-piece is made an erecting one capable of bringing the image into focus. This adds much to the convenience, but not to

the correctness of the method. Thus the conditions of the microscopic object-glass are reversed, the principal focus being transferred from the front to the back, and the rays estimated are those of the extreme oblique pencils, which may or may not pass through the point of the principal focus of the glass when used for the microscope.

The importance of this in the illumination of objects immediately suggested itself; and the author obtained a further proof by another experiment bearing directly upon this point. A blackened wire was placed under a microscope at the focal point, with an object-glass of considerable power and aperture, the wire covering the field with the eye-piece used. The field was then illuminated with an achromatic condenser, the field of illumination exceeding, as it usually does, that of the microscope. As was expected, the oblique rays which passed on both sides of the wire prevented its blackness from being seen (this becoming of a milky-grey), until the field of illumination was reduced to the extent of that of the microscope, when it immediately assumed to the eye its natural blackness. This reminded the author of a beautiful illustration given by Professor Faraday some years since at the Royal Institution, of the effect of glare produced by placing white muslin blackened in parts before a white paper printed in large letters; with the white muslin in front, the letters were scarcely visible, while through the blackened parts they resumed their natural appearance. These experiments suggested the new method adopted, which may be briefly stated as follows:—

The microscope of which the object-glass is to be examined is placed horizontally and centred by an object placed in the focus. Next, there is substituted in place of the eye-piece, a hollow cone with an aperture at its summit. Light passing through this aperture is made to form an image of it in the principal focus of the object-glass, in the place of the original object. On this image a horizontally placed examining microscope is then directed, which traverses as the radius of a graduated circle, having its centre corresponding with the place of the original object, and therefore with the image to be viewed; and the angle of aperture is measured by the arc passed through between two extreme positions, in the usual manner. The method is further explained in the paper by a figure and description of the apparatus, which was itself exhibited in the Library after the meeting.

March 16, 1854.

CHARLES WHEATSTONE, Esq., V.P., in the Chair.

The following paper was read:—

"On some new Compounds of Phenyl." By A. WILLIAMSON, Ph.D., F.C.S., Professor of Practical Chemistry in University College. Communicated by Dr. SHARPEY, Sec. R.S. Received March 15, 1854.

This communication contains a notice of some of the results obtained in an investigation of Carbolic Acid or Hydrated Oxide of Phenyl, conducted, under the author's superintendence, by Mr. Scrugham in the Analytical Laboratory of University College.

Referring to the substitution products obtained by Laurent from hydrate of phenyl by the action of chlorine and bromine, as well as to its combination with acids prepared by that chemist in conjunction with Gerhardt, the author states that the substance which they conceived to be chloride of phenyl has been found by Mr. Scrugham to be a mixture of two compounds.

As regards the preparation of hydrate of phenyl from the creosote of coal-tar, it is observed that the numerous fractional distillations by which it is usually isolated may be abridged by crystallization; for if creosote, having the boiling-point between 186° and 188° Cent., be left for some time in contact with a few crystals of the pure hydrate, it deposits a considerable quantity of beautiful colourless needles, which, when separated from the mother-liquid, distil at 184° Cent., and condense in the neck of the retort into a solid mass of pure hydrate of phenyl.

When pentachloride of phosphorus is added to hydrate of phenyl, the action is at first very energetic, hydrochloric acid being evolved, and the mixture becoming hot; but after a time the addition of fresh portions of pentachloride produces no perceptible action, unless the mixture be heated. Oxychloride of phosphorus is formed, as well as

a neutral oily body, which is insoluble in aqueous potash at the common temperature, but soluble with decomposition in boiling potash. This oily compound would, from its mode of formation, be naturally supposed to be the chloride of phenyl, and it has been so considered by some distinguished chemists. It may, however, be separated by distillation into two perfectly definite and distinct bodies, one of which boils at 136° Cent., the other at a temperature above the range of mercurial thermometers. The former of these is a colourless mobile liquid, possessing a fragrant smell, not unlike that of bitter almonds. The latter is a more consistent inodorous liquid, which solidifies at a low temperature into a mass of colourless crystals. The liquid having the boiling-point of 136° is nothing else than the *chloride of phenyl*. The crystalline body is the *phosphate of phenyl*, one of the most beautiful products in organic chemistry. In the liquid state it is slightly yellow by transmitted light, and it reflects the more refrangible rays with a fine opalescent appearance, due no doubt to the so-called epipolic refraction. The epipolic rays visible by ordinary daylight on and at some depth below its surface, are of a fine violet tint, differing decidedly from the blue colour exhibited by disulphate of quinine in like circumstances. The flame of sulphur does not bring out this effect more strongly than the diffused light of the sun.

Phosphate of phenyl dissolves in strong nitric acid with evolution of considerable heat, and the solution gives out nitrous fumes on ebullition. A heavy yellow oil is precipitated by water from this solution, and collects in drops which ultimately solidify, and their solidification is, singularly enough, accelerated by *hot* water, by reason of its more quickly dissolving out the nitric acid which at first holds the solid body in solution. *Nitrophosphate of phenyl* is an acid, and forms with potash a beautiful crystalline salt.

An alcoholic solution of phosphate of phenyl decomposes acetate of potash on ebullition. After the alcohol is distilled off, the temperature of the mixture rises rapidly on the application of further heat, and a limpid oleaginous substance, having a very peculiar odour, distils over, which possesses the composition of *acetate of phenyl*. This compound boils at 190° Cent.; it is heavier than water, and very slightly soluble in that liquid. It dissolves with decomposition in boiling potash.

Cyanide of phenyl is obtained by the action of the phosphate on cyanide of potassium. It is decomposed by boiling potash with evolution of ammonia.

Terchloride of phosphorus, when distilled with hydrate of phenyl, seems to act at first similarly to the pentachloride, but the phosphite of phenyl formed is decomposed by heat; and among the products of distillation is found a body boiling at 80° Cent., and possessing all the properties of *benzin*, i. e. *hydruret of phenyl.*

The formation of the *iodide of phenyl* is necessarily attended with some difficulty, owing to the circumstance of phosphorus not combining with more than three equivalents of iodine. Its boiling-point is 190° Cent.

Mr. Scrugham has had reason to confirm the statements of Laurent and Gerhardt respecting the *benzoate of phenyl*, and has prepared that compound in considerable quantities by the action of chloride of benzoyle on phenylate of potash. Chloride and phosphate of phenyl could not be made to react on benzoate of potash.

Chloride of cuminyl reacts with violence on phenylate of potash, with formation of *cuminate of phenyl*, a compound analogous to the benzoate.

Chloride of phenyl was heated with phenylate of sodium, with a view to the formation of *oxide of phenyl*, and there is no doubt that this compound was formed by the reaction, as the correlative product, chloride of sodium, was detected. But a further account of this and other reactions is deferred until the experimental investigation is more advanced.

Specimens of most of the compounds mentioned were exhibited.

March 23, 1854.

Colonel SABINE, R.A., Treas. and V.P., in the Chair.

The following paper was read :—

"Note on an indication of depth of Primæval Seas, afforded by the remains of colour in Fossil Testacea." By EDWARD FORBES, F.R.S., Pres. G.S. &c. Received March 22, 1854.

When engaged in the investigation of the bathymetrical distribution of existing mollusks, the author found that not only did the colour of their shells cease to be strongly marked at considerable depths, but also that well-defined patterns were, with very few and slight exceptions, presented only by testacea inhabiting the littoral, circumlittoral and median zones. In the Mediterranean only one in eighteen of the shells taken from below 100 fathoms exhibited any markings of colour, and even the few that did so, were questionable inhabitants of those depths. Between 35 and 55 fathoms, the proportion of marked to plain shells was rather less than one in three, and between the sea-margin and 2 fathoms the striped or mottled species exceeded one-half of the total number.

In our own seas the author observes that testacea taken from below 100 fathoms, even when they were individuals of species vividly striped or banded in shallower zones, are quite white or colourless. Between 60 and 80 fathoms, striping and banding are rarely presented by our shells, especially in the northern provinces ; and from 50 fathoms shallow-wards, colours and patterns are well marked.

The relation of these arrangements of colour to the degrees of light penetrating the different zones of depth, is a subject well worthy of minute inquiry, and has not yet been investigated by natural philosophers.

The purpose in this brief notice is not, however, to pursue this kind of research, but to put on record an application of our knowledge of the fact that vivid patterns are not presented by testacea living below certain depths, to the indication of the depth, within certain limits, of palæozoic seas, through an examination of the traces of colour afforded by fossil remains of testacea.

Although their original colour is very rarely exhibited by fossil shells, occasionally we meet with specimens in which, owing probably to organic differences in the minute structure of the coloured and colourless portions of the shell, the pattern of the original painting is clearly distinguished from the ground tint. Not a few examples are found in Mesozoic as well as in Tertiary strata, but in all the instances on record, the association of species, mostly closely allied to existing types, and the habits of the animals of the genera to which they belong, are such as to prevent our having much difficulty about ascertaining the probable bathymetrical zone of the sea in which they lived.

But in palæozoic strata the general assemblage of articulate, molluscan and radiate forms is so different from any now existing with which we can compare it, and so few species of generic types still remaining are presented for our guidance, that in many instances we can scarcely venture to infer with safety the original bathymetrical zone of a deposit from its fossil contents. Consequently any fact that will help us in elucidating this point becomes of considerable importance.

Traces of colouring are rarely presented by palæozoic fossils, and the author knows of few examples in which they have been noticed. Professor Phillips, in his 'Geology of Yorkshire,' represents the carboniferous species, *Pleurotomaria flammigera* (i. e. *carinata*) and *conica*, as marked with colour, and Sowerby has figured such markings in *P. carinata* and *P. rotundata*. In the excellent monograph of the carboniferous fossils of Belgium, by Professor De Koninck of Liège, indications of pattern-colouring are faintly shown in the figures of *Solarium pentangulatum*, and distinctly in those of *Pleurotomaria carinata* and *Patella solaris*.

In the cabinets of the Geological Survey of Great Britain are some finely-preserved fossils from the carboniferous limestone of Parkhill, near Longnor in Derbyshire. Among these are several

that present unmistakeable pattern-markings, evidently derived from the original colouring. They are—

Pleurotomaria carinata and *conica,* showing wavy blotches, resembling the colouring of many recent *Trochidæ.*

An undescribed *Trochus,* showing a spiral band of colour.

Metoptoma pileus, and

Patella ? retrorsa, both with radiating stripes, such as are presented by numerous existing *Patellidæ.*

Natica plicistria, with broad mottled bands.

Aviculo-pecten, a large unnamed species, with spotty markings on the ribs in the manner of many existing *Pectines.*

Aviculo-pecten sublobatus, Ph. ? Beautifully marked with radiating, well-defined stripes, varying in each individual, and resembling the patterns presented by those recent *Aviculæ* that inhabit shallows and moderate depths.

Aviculo-pecten intercostatus and *elongatus* also exhibit markings.

Spirifer decorus and *Orthis resupinata,* show fine radiating white lines.

Terebratula hastata, with radiating stripes.

The analogy of any existing forms that can be compared with those enumerated, would lead to the conclusion that the markings in these instances are characteristic of mollusks living in a less depth of water than 50 fathoms. In the case of the *Terebratula,* which belongs to a genus the majority of whose living representatives inhabit deep water, it may be noticed that all the living species exhibiting striped shells are exceptions to the rule, and come from shallow water.

There are many circumstances which warrant us to suspect that the carboniferous mountain limestone of most regions was a deposit in shallow water. The facts now adduced materially strengthen this inference.

In the British Museum there is a beautifully spotted example of a Devonian *Terebratula,* brought by Sir John Richardson from Boreal America.

Specimens of the *Turbo rupestris,* from the Lower Silurian Limestone of the Chair of Kildare near Dublin, exhibit appearances that seem to indicate spiral bands of colour.

March 30, 1854.

THOMAS BELL, Esq., V.P., in the Chair.

The following papers were read:—

I. "Note on the Melting-point and Transformations of Sulphur." By B. C. Brodie, Esq., F.R.S. Received March 30, 1854.

In the treatises of chemistry where the results of different observers are collected, various statements will be found as to the melting-point of sulphur. The numbers given in Gmelin's Chemistry vary from 104°·5 C. to 112°·2 C., but of five chemists cited, no two agree as to this apparently simple fact. There is evidently some peculiarity about this melting-point which is the cause of these anomalous results. In some experiments on allotropic substances, in which I have been engaged, I had occasion to submit this question to a more searching inquiry than it had hitherto received, in which I have discovered the cause of these discrepancies. In the present note I will briefly give the results at which I have arrived, reserving the details for a further and more full communication.

The melting-point of sulphur varies according to its allotropic condition. This condition is readily altered by heat, and invariably, without peculiar precautions, by melting. Hence the temperature at which sulphur melts is different from that at which it will solidify, or at which, having been melted, it will melt again.

The melting-point of the octohedral sulphur, as crystallized from the bisulphide of carbon, is 114°·5 C. But from the facility with which this sulphur, when heated even below its melting-point, passes into the sulphur of the oblique system, this fact may readily be overlooked. When this sulphur, in the state of fine powder, is heated even for the shortest time between 100° and 114°·5, this change cannot be avoided. For the transformation of large crystals

a longer time is required. At a certain point the crystal becomes opake, and is often broken in pieces at the moment of the change. When in such a crystal this change has either entirely or partially taken place, the melting-point will be above 114°·5. The minute crystals of sulphur from alcohol, which are so extremely thin that their angles cannot be measured, have this melting-point of 114°·5, which fixes the system to which the crystals belong. The crystals of sulphur from benzole (rectified coal naphtha) melt also at 114°·5. The crystals from alcohol are very minute, consequently so readily transformed, that they presented anomalies which led me to doubt whether sulphur of both forms did not exist among them. I answered this question by dividing a certain number of carefully selected crystals, and taking the melting-point of the two halves of the same crystal. I found that these melting-points in many cases did not correspond, which would have been the case if the anomalies had arisen from the different nature of the crystals. Sulphur which has been melted at 114°·5, and of which the temperature has not been raised above 115°, remains, on solidification, perfectly transparent for any length of time. Heated beyond this point, it becomes, on cooling, more or less opake.

When sulphur has been converted by heating for a sufficient length of time, in the manner above mentioned, between 100° and 114°·5, it acquires a fixed melting-point of 120° C. This is the melting-point of the oblique prismatic sulphur. If sulphur thus converted be carefully melted so as to raise the temperature as little as possible above the melting-point, no sensible difference will be observed between the point of melting and of solidification. To obtain this fixed melting-point of 120°, care must be taken that the transformation of the sulphur has been thoroughly effected. If this be not done, it may melt at any point between 114°·5 and 120°. If, however, the temperature of the melted sulphur be raised above its melting-point of 120°, the point of solidification will be altered, and may lie even below the first melting-point of 114°·5 *. The point of solidification is in this case not fixed, but depends upon the temperature to which the sul-

* This has been observed by Person, who states that if sulphur be heated above 150° its melting-point is lowered to about 112° or 110°. He says, that when heated with care, the thermometer will remain constant during crystallization, at 115°. I have not found this correct.—Ann. de Chemie, vol. xxi. p. 323.

phur is raised and upon the mode in which it is cooled. It has varied in my experiments from 118° to as low as 111°. When the melting-point of the sulphur, thus solidified, is taken, it will begin to melt at about the temperature of solidification. The cause of this anomaly is evident. When the temperature of sulphur is raised above 120°, a transformation into the viscid form instantly commences, so that the sulphur is a mixture of the two varieties, and the melting-point varies according to the proportion in which these two varieties are mixed. It varies inversely with the temperature to which the sulphur is raised, so that the presence of the viscid sulphur lowers the point of solidification. There is, however, a limit beyond which the melting-point is not affected by this admixture. I made the experiment of pouring sulphur, heated to its boiling-point, into water of different temperatures, and of taking the melting-point of the sulphur when it had become hard. Five different preparations, which, when extracted with bisulphide of carbon, gave each a different quantity of insoluble sulphur, coincided in the melting-point of about 112°. This sulphur, before melting, becomes transparent, and passes again into the viscid or elastic condition.

The sulphur which is insoluble in bisulphide of carbon, and which is prepared by extracting the hardened viscid sulphur with that reagent, has a melting-point considerably above 120°, but which I have not been able to determine with precision.

I had placed in a water-bath, at 100°, tubes containing fragments of the three definite varieties of sulphur. After a short time, on examining the tubes, I found the insoluble sulphur, which I have stated to have such a high melting-point, distinctly melted. The octohedral sulphur had become opake and rounded at the edges, the other was unaltered in appearance. Further inquiry convinced me that the cause of the melting of the insoluble sulphur was, that it had passed into another modification, and that this conversion was attended with evolution of heat sufficient to melt the sulphur. The insoluble sulphur thus converted remains transparent, and is perfectly soluble in bisulphide of carbon.

It is stated in chemical treatises that the opacity which on solidification comes over the melted sulphur, is due to the transformation of the oblique prismatic into the octohedral sulphur, and the consequent disruption of the crystal. To this cause also is attributed the

evolution of heat which has been observed in solid sulphur immediately after cooling. There are, however, no sufficient grounds for this view, and some of the observations which I have given are decidedly adverse to it. 1. The change readily takes place, even at temperatures at which sulphur becomes opake, in the opposite direction, namely, from the octohedron to the oblique prism. 2. The melting-point of the opake sulphur coincides too nearly with its point of solidification for it to be supposed that this change in it has taken place. On extracting melted sulphur which had become opake, with bisulphide of carbon, I have constantly found present traces of insoluble matter, even where the greatest precaution had been taken to avoid elevation of temperature; and this opacity appears to me to be due to the hardening of the viscid sulphur, and the consequent deposition of opake matter in the pores of the crystals, which is quite sufficient to account for it. It remains to ascertain the cause of the evolution of the heat. On this point also I will offer a suggestion. It is well known that the appearance of opacity is delayed by pouring the sulphur into cold water, and that the sulphur thus formed is at first viscid and transparent, and only after a time becomes solid and opake. The received view, I believe, is that the hard sulphur thus formed is the solid form of the viscid sulphur, in the same sense as ice is the solid form of water. It appears to me more probable that these two sulphurs stand in a different relation, and that the change which takes place on solidification is an allotropic transformation of the viscid sulphur into the insoluble sulphur and one of the other modifications. In the case of sulphur gradually cooled this change takes place with rapidity, and, like other similar transformations, is attended with a sensible evolution of heat. Where the sulphur is *tempered* the change takes place very slowly, and the heat evolved is not perceived. This view is confirmed by a fact which I have discovered, namely, that the viscid sulphur possesses another solid form. I have found that when sulphur, melted at a high temperature, is suddenly exposed to intense cold—the cold of solid carbonic acid and ether—the sulphur formed is not viscid, but solid, hard, and perfectly transparent. When the temperature is allowed to rise to that of the air, the sulphur becomes soft and elastic. It is probable that this is the true solid form of the viscid sulphur.

II. "On the Structure and Affinities of *Trigonocarpon* (a fossil fruit of the Coal-measures)." By JOSEPH D. HOOKER, M.D., F.R.S. Received March 23, 1854.

Having been for some time engaged in examining the structure and affinities of some fossil fruits of the coal formation, included under the name *Trigonocarpon*, and the progress which I am enabled to make being extremely slow (owing to the difficulty of procuring good specimens), I am induced to lay before the Royal Society such results as I have arrived at, for publication in their Proceedings (if thought worthy of that honour). The details and illustrations of the subject will, when complete, be offered to the Geological Society of London.

My attention has for many years been directed to the genus *Trigonocarpon*; as, from the period of my earliest acquaintance with the flora of the carboniferous epoch, I have felt assured, that botanically, this was the most interesting and important fossil which it contained in any great abundance, and that until the affinities of this were determined, the real nature of the flora in question could never be regarded as even approximately ascertained.

In the first place, *Trigonocarpon* is so abundant throughout the coal-measures, that in certain localities some species may be procured by the bushel; nor is there any part of the formation in which they do not occur, except the underclays and limestone. The sandstone, ironstones, shales and coal itself, all contain them.

Secondly. The symmetry in form and size which many of them display, the regularity of the sculpturing on their surfaces, and various other points, suggested their belonging to a class of highly organized vegetables.

Thirdly. The fact of our being wholly unacquainted with the organs of fructification belonging to the exogenous vegetation, which also abounds in the coal formation, coupled with the assumed highly organized nature of *Trigonocarpon*, favoured the assumption that these might throw light upon one another, and seemed to afford a legitimate basis upon which to proceed, should I ever procure specimens of *Trigonocarpon* displaying structure, which I had long hoped to do.

It is, however, only since my return from India that I have been so fortunate as to obtain good specimens, and for these I am indebted to my friend Mr. Binney of Manchester, who has himself thrown much light upon the vegetation of the coal epoch, and whose exertions indeed have alone enabled me to prosecute the subject; since he has not only placed his whole collection of Trigonocarpons at my disposal, but has shared with me the trouble and expense of their preparation for study. All the specimens were found imbedded in a very tough and hard black-band or clay ironstone, full of fragments of vegetable matter, and which appears originally to have been a fine tenacious clay.

The individual Trigonocarpons are exposed by breaking this rock, and are invariably so intimately adherent to the matrix as to be fractured with it. A great many of these lumps of ironstone, containing partially exposed Trigonocarpons, have been sliced by a lapidary in the usual manner, and excessively thin sections taken on slips of glass. The sections were made necessarily very much at random, but as nearly as possible parallel, or at right angles to the long diameter of the fruit. Five of the specimens thus operated upon have proved instructive, presenting the same appearances, and all being intelligible, and referable to one highly developed type of plants. As, however, the term highly developed may appear ambiguous, especially with reference to a higher or lower degree in the scale of vegetable life, I may mention that by this term I mean to imply that there are in the fruit of *Trigonocarpon* extensive modifications of elementary organs, for the purpose of their adaptation to special functions, and that these modifications are as great, and the adaptation as special, as any to be found amongst analogous fruits in the existing vegetable world.

Thus, I find that the integuments of the fruit of *Trigonocarpon* are each of them a special highly organized structure; they are modifications of the several coats of one ovule, and indeed of the same number of integuments as now prevail in the ovules of living plants.

The number, structure and superposition of these, are strongly indicative of the Trigonocarpons having belonged to that large section of existing coniferous plants, which bear fleshy, solitary fruits, and not cones; and they so strongly resemble the various parts of the fruit of the Chinese genus *Salisburia*, that, in the present state of our

knowledge, it appears legitimate to assume their relationship to it. In all the five specimens alluded to, there are more or less perfect evidences of four distinct integuments, and of a large cavity, which is in all filled with carbonate of lime and magnesia; these minerals, I presume, having replaced the albumen and embryo of the seed.

The general form of the perfect fruit is an elongated ovoid (rather larger than a hazel nut), of which the broader or lower end presents the point of attachment, while the upper or smaller end is produced into a straight, conical, truncated rostrum or beak, which is perforated by a straight longitudinal canal. The exterior integument is very thick and cellular, and was no doubt once fleshy; it alone is produced beyond the seed and forms the beak; its apex I assume to have been that of the primine of the ovule, and its cavity the exostome. The second coat appears to have been much thinner, but hard and woody or bony; it is impervious at the apex; is also ovoid, and sessile by its broad base within the outer integument, with which it is perhaps adherent everywhere except at the apex. This is marked by three angles or ridges, and being that alone which (owing to its hard nature) commonly remains in the fossil state, has suggested the name of *Trigonocarpon.* Within this are the third and fourth coats, both of which are very delicate membranes; one appears to have been in close apposition with the inner wall of the second integument, and the other to have surrounded the albumen. These are now separated both from one another, and from the inner wall of the cavity, by the shrinking of the contents of the latter, and the subsequent infiltration of water charged with mineral matter. I may remark, however, that these two membranes may be due to the separation of one into two plates, in which case the original one was formed of several layers of cells. Hitherto I have not been able to trace any organized structure within the cavity of the fruit, and its real nature therefore remains doubtful. It is only from the strong resemblance, in structure, appearance and superposition, which these integuments present to those of Taxoid coniferæ, that I assume their probable relationship. *Salisburia,* especially, has the same ovoid fruit, sessile by its broader end, and its outer coat is perfectly analogous, being thick, fleshy, and perforated at its apex by a longitudinal canal (the exostome of the ovule); within this is a perfectly similar, woody, two or three angled, impervious integument, form-

ing the nut. This again is lined with one very delicate membrane, and contains a mass of albumen covered with a second similar membrane. A marked analogy is presented to the European botanist by the fruit of the Yew, which has the same integuments though somewhat modified; the outer, fleshy coat in the Yew is however a cup-shaped receptacle, and not drawn up over the nut so as to leave only a small canal at the top, as in *Salisburia* and *Trigonocarpon*. The nut also does not adhere to the fleshy cup except below its middle. The internal structure is the same in all three.

Such are the main facts which I have been able satisfactorily to establish. There are many others yet to be worked out, especially those connected with the individual tissues of which those bodies are composed; and it is particularly to be borne in mind that the discovery of some structure indicative of albumen or embryo, is absolutely essential to the complete establishment of the affinity I have suggested.

It must not be overlooked, that the characters through which I have attempted to establish an affinity between *Trigonocarpon* and Coniferæ are equally common to the fruits of Cycadeæ; and in connexion with this subject I may remark, that M. Brongniart* has referred the genus *Noggerathia*, which is also found in the coal-measures, to that natural order, together with some associated organs which are probably Trigonocarpons in a mutilated state. The leaves of *Noggerathia* are, however, alone known, and Dr. Lindley, when figuring those of one species (Lindley and Hutton, Fossil Flora, 28, 29), pointed out their great resemblance to those of *Salisburia*, thus affording collateral evidence of the view I have been led to adopt from an examination of the fruit alone.

* Annales des Sciences Naturelles, 2nd Series, vol. v. p. 52.

April 6, 1854.

THOMAS GRAHAM, Esq., V.P., in the Chair.

Notice was given that at the next Meeting of the Society, Lord Ashburton would be proposed for immediate ballot, to which, as a Peer of the Realm, his Lordship is entitled.

The following communications were read:—

I. "On a peculiar Arrangement of the Sanguiferous System in *Terebratula* and certain other BRACHIOPODA." By W. B. CARPENTER, M.D., F.R.S. Received March 30, 1854.

In a memoir "On the Minute Structure of Shell," read before the Royal Society January 17, 1843, (and subsequently embodied in a "Report" on the same subject, prepared at the request of the British Association for the Advancement of Science, and published in its Transactions for 1844,) I first announced the fact, that the 'punctations' which had been previously noticed on the exterior of many Brachiopodous shells, both recent and fossil, are really the orifices of *tubular perforations*, which pass directly through each valve, from one of its surfaces to the other (fig. 1).

Having subsequently obtained specimens of *Terebratulæ* in which the soft parts of the animals had been preserved, in connection with their shells, I ascertained that these passages are occupied in the living state by membranous cæca, *closed externally*, but opening on the *internal* surface of the shell, and filled with minute cells of a brownish hue. Recollecting that Professor Owen, in his account of dissections of some species of *Terebratula* and *Orbicula* (Transactions of the Zoological Society, vol. i.), had spoken of an unusual adhesion of the mantle to the shell in these Bivalves, it occurred to me that this adhesion might be due to a continuity between the

mantle and these cæcal tubuli; and I carefully sought for evidence of such a structure. In this, however, I was entirely unsuccessful; for the mantle, when stripped from the shell, presented no appearance whatever of having transmitted any such prolongations into its substance; on the contrary, it was evidently continued over the mouths of the cæca with which it was in apposition; and I frequently found its external surface (*that* in contact with the shell) covered *in patches* with cells exactly resembling in size and aspect those contained within the cæca. I was equally unsuccessful in the attempt to trace any other connection between these cæca and the soft parts of the animal; so that, although their importance in its œconomy scarcely admitted of doubt, the nature of their function remained entirely unknown. The idea that they had any connection with the formation of the *shell* itself, seemed to be completely negatived by the fact, that in a large proportion of the group of Brachiopoda, no such perforations exist; notwithstanding that their shells, in every other feature of minute structure, are exactly accordant with that of *Terebratula*.—The foregoing results were communicated to the British Scientific Association in 1847, and were embodied in the Second Part of my "Report" published in its Transactions for that year.

The physiological importance of the characters of 'perforation' or 'non-perforation' has become continually more obvious, as the principles on which the subdivision of the group of Brachiopoda should be founded, have been gradually settled by those who have concerned themselves with its systematic arrangement; and in particular, the *universal presence* of the perforations in the shells of the family *Terebratulidæ*, contrasted with their equally universal absence in those of the family *Rhynchonellidæ*, unequivocally marked its relation to the general conformation of the *animals* of these subdivisions.

Having been requested by Mr. Davidson to undertake a more detailed investigation than I had yet made, into the minute structure of the shells of Brachiopoda, for the sake of throwing still further light upon the classification of the group, I applied myself afresh to the solution of the problem, and believing that I have succeeded in ascertaining the import of this curious feature in the organization of Terebratula and its allies, I beg to offer an account of my results to the Royal Society.

The membrane which is commonly spoken of as 'the mantle,' and which may be stripped from the shell by the use of sufficient force to overcome its adhesions, must, I maintain, be considered as really its *inner layer* only; for I find that an outer layer exists, so intimately incorporated with the shell as not to be separable from it without the removal of its calcareous component by maceration in dilute acid. When thus detached, this outer layer is found to be continuous with the membrane lining the perforations in the shell (fig. 1 *b*); so that their tubular cæca are, in fact, prolongations of the *real* external surface of the mantle. The adhesion of the *inner* to the *outer*

Fig. 1.

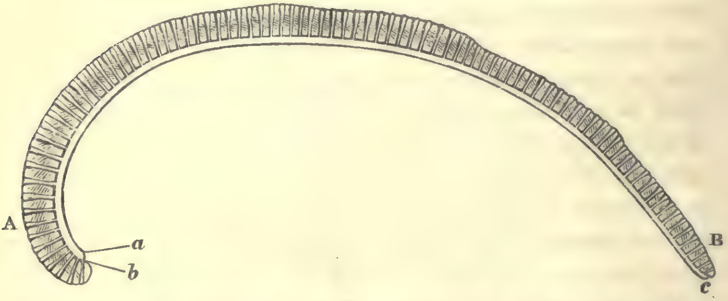
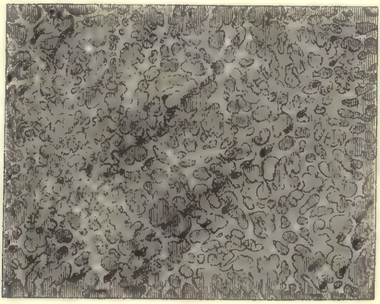


Diagram of the intra-palleal sinus-system of *Terebratula*, with its cæcal prolongations into the shell;—A, B, section of valve; *a*, inner layer of mantle, *b*, outer layer in contact with the shell, and giving off cæca; *c*, continuity of the two at margin of valve.

layer (which Professor Owen, not being aware of the existence of an outer layer, interpreted as an adhesion of the *mantle* to the *shell*) does not extend to the whole of the contiguous surfaces, but is limited to certain bands or spots,—the two layers of membrane, in the intervals between these, being separated by a set of irregular spaces, freely communicating with one another, and with the cavities of the cæca, so as to form a rude network. This arrangement is peculiarly well marked in *Terebratula caput-serpentis*, as shown in the figure (fig. 2); and to those who are familiar with the condition of the circulating apparatus in the inferior Mollusca, it is scarcely possible not to recognize in it a 'sinus-system,' corresponding to that which is formed in the Tunicata by the partial adhesion of the second and third tunics to each other.

Considered under this point of view, the cæcal structure (as was first suggested to me by my friend Mr. T. H. Huxley) bears a close resemblance to the vascular prolongations, which, in many Ascidians, pass from the sinus-system into the substance of the 'test;' the chief difference lying in this,—that whilst each of the vascular prolongations into the 'test' of the Ascidians contains both an *afferent* and an *efferent* canal,—no such distinction ordinarily manifests itself in these prolongations of the intra-palleal sinus-system of *Terebratula,* although I have met with indications of it in *Crania.* Their cæcal character, however, is by no means opposed to the views I am now giving of their physiological nature; for it has been shown by M. de Quatrefages, that the prolongations of the 'general cavity of the body,' which pass into the branchiæ and other appendages of Annelida, transmitting to them its nutritive fluid for aëration, are always cæcal, notwithstanding that they are sometimes distributed as minutely as blood-vessels*.

Fig. 2.



Sinus-system of *Terebratula caput-serpentis* (as shown by the grinding away of the shell, without detaching the mantle), being a network of canals formed by the adhesion of the two layers of the mantle at certain spots, leaving passages around them.

On this interpretation, the cells which are found within the cæca, and in the spaces between the contiguous surfaces of the two layers of the mantle, are to be regarded as *blood-corpuscles,* and they correspond in size and appearance (so far as can be determined by specimens preserved in spirits) with the blood-corpuscles of Ascidian and Lamellibranchiate Mollusks.

* Ann. des Sci. Nat., 3e sér., Zool., tom. xviii. p. 307.

The sinus-system from which this collection of cæca proceeds, appears to be altogether distinct from the vascular apparatus of the (so-called) 'mantle,' (that is, according to my interpretation, *of the inner layer* of the mantle) which has been described by Professor Owen; but it probably communicates with the 'common sinus' at the back part of the visceral chamber, which is stated by Professor Owen to receive the blood, not only from the palleal sinuses of the dorsal and ventral valves, but also from "other sinuses that there fill, line, and seem to form, the visceral or peritoneal cavity*."

It cannot be deemed improbable, then, that the apparatus in question is *branchial* in its nature; and that it is designed to provide for certain tribes a more special means of aërating the blood, than is afforded by that distribution of blood to the general surface of the mantle, which is common to the entire group. This view of its respiratory office is confirmed by an observation communicated to me by Professor Quekett; viz. that the discoidal opercula which cover the external orifices of the cæca, and which, though adherent to the periostracum, are not structurally continuous with it, present appearances in young shells, which seem indicative of the existence of a fringe of cilia round each, designed to produce currents of water over the extremities of the cæca.

The resemblance which these cæcal prolongations of the sinus-system into the shell of the *Terebratula* bear to the vascular prolongations of the sinus-system into the test of certain *Ascidians*, is not without its parallel in another group, which (as pointed out by Mr. Hancock, Ann. of Nat. Hist. vol. v. p. 198) is intimately related to that of Brachiopoda,—namely, the *Bryozoa*. The stony walls of the 'cells' which invest the soft bodies of many species of *Eschara*, *Lepralia*, &c., are marked, like the shells of Terebratulæ, with punctations, which are really the orifices of short passages extending into them from their internal cavity, as sections of these structures demonstrate. These passages I have found to be occupied by prolongations of the visceral sac, which is the only representative of a circulating system among these animals; and they thus convey the nutrient fluid which this contains, into the substance of the framework formed by the calcified tunics of these animals.

* See Mr. Davidson's Monograph on the "British Fossil Brachiopoda," published by the Palæontographical Society, vol. i. p. 15.

I need not here enlarge upon the additional value which these structural and physiological considerations afford, to the character of "perforation" or "non-perforation" in the shells of Brachiopoda. The importance of this character in systematic arrangement will plainly appear, I think, from the details which I have published in the Introduction to Mr. Davidson's Monograph already referred to.

II. "On a new Series of Sulphuretted Acids." By Dr. August Kekulé. Communicated by Dr. Sharpey, Sec. R.S. Received April 5, 1854.

Adopting the idea that the series of organic compounds of which sulphuretted hydrogen is the type, corresponds in every respect with the series of which water is the type, I concluded that not only mercaptans and neutral sulphides which correspond to the alcohols and ethers, but also compounds corresponding to the acids, anhydrous acids and ethers of acids might be produced; I therefore endeavoured to obtain reactions which would enable me to replace oxygen in the compounds of the latter series by sulphur.

Such reactions are produced by the compounds of sulphur with phosphorus—the tersulphide ($P_2 S_3$) and the pentasulphide ($P_2 S_5$)—which are easily obtained by fusing together amorphous phosphorus and sulphur in an atmosphere of carbonic acid; no explosion takes place, although the combination is attended with a very violent action.

Experiment has proved that these combinations of sulphur and phosphorus act on the members of the series of water in the same manner (although less violently) as the corresponding compounds of chlorine and phosphorus;—however, with this difference, that by using the chlorine compounds the product is resolved into *two* groups of atoms, while by using the sulphur compounds there is obtained only *one* group; a peculiarity, which, according to the bibasic nature of sulphur, must have been expected. By acting on these compounds of sulphur and phosphorus with water one atom of sulphu-

retted hydrogen is obtained, while the chlorides give two atoms of hydrochloric acid,

$$6\left.\begin{matrix}H\\H\end{matrix}\right\}O+P_2\,S=3\left.\begin{matrix}H\\H\end{matrix}\right\}S+2PO_3\,H_3$$

$$6\left.\begin{matrix}H\\H\end{matrix}\right\}O+2P\,Cl_3=6H\,Cl+2PO_3\,H_3.$$

Similar reactions are observed with organic compounds belonging to the series of water with the formation of phosphorous and phosphoric acids respectively, or a copulated acid. By acting in this way, the following series of sulphuretted organic compounds is obtained, by the side of which are placed for comparison the products formed by the action of the chlorides of phosphorus on the same substances.

Sulphuretted Hydrogen. $\left.\begin{matrix}H\\H\end{matrix}\right\}S.$	Hydrochloric Acid. $2H\,Cl.$
Mercaptan. $\left.\begin{matrix}C_2\,H_5\\H\end{matrix}\right\}S.$	Chloride of Ethyl+Hydrochloric Acid. $C_2\,H_5\,Cl+H\,Cl.$
Sulphide of Ethyl. $\left.\begin{matrix}C_2\,H_5\\C_2\,H_5\end{matrix}\right\}S.$	Chloride of Ethyl. $2C_2\,H_5\,Cl.$
Othyl-Hydrosulphuric Acid. $\left.\begin{matrix}C_2\,H_3\,O\\H\end{matrix}\right\}S.$	Chloride of Othyl+Hydrochloric Acid. $C_2\,H_3\,O,Cl+H\,Cl.$
Othyl-Sulphide of Othyl. $\left.\begin{matrix}C_2\,H_3\,O\\C_2\,H_3\,O\end{matrix}\right\}S.$	Chloride of Othyl. $2C_2\,H_3\,O,Cl.$
Othyl-Sulphide of Ethyl. $\left.\begin{matrix}C_2\,H_3\,O\\C_2\,H_5\end{matrix}\right\}S.$	Chloride of Othyl+Chloride of Ethyl. $C_2\,H_3\,O,Cl+C_2\,H_5\,Cl.$

Mercaptan is obtained by the action of tersulphide or pentasulphide of phosphorus on alcohol with extreme facility. Sulphide of ethyl may also be prepared by acting on ether in a similar manner.

Thiacetic Acid,—Sulphuretted Acetic Acid,—has been obtained by me by acting on monohydrated acetic acid with tersulphide of phosphorus. It is a colourless liquid, boiling at about 93° C., and has a peculiar odour resembling sulphuretted hydrogen and acetic

acid. It dissolves potassium in the cold and zinc on heating with the evolution of hydrogen, and gives with lead a salt less soluble than the ordinary acetate, so that it gives a precipitate with acetate of lead. By recrystallization from water or alcohol, the lead salt is obtained in fine silky needles, which, though quite colourless at first, are rapidly decomposed (whether in solution or in the solid form) with the formation of sulphide of lead.

By analysis I found the lead salt contained—

Lead 58·8 per cent. Theory requires 58·0 per cent.

The acid contained—

Sulphur 41·3 per cent. Theory requires 42·1 per cent.

Thiacetic acid is also formed in small quantity and by secondary action, by distilling pentasulphide of phosphorus with fused acetate of soda. Pentachloride of phosphorus gives a violent reaction with thiacetic acid, yielding sulphochloride of phosphorus, chloride of othyl, and hydrochloric acid,

$$\left.\begin{matrix} C_2H_3O \\ H \end{matrix}\right\} S + PCl_5 = C_2H_3OCl + HCl + PSCl_3.$$

Thiacetate of Othyl.—Sulphide of Othyl.—Anhydrous Sulphuretted Acetic Acid.—Pentasulphide of phosphorus acts but very feebly upon anhydrous acetic acid in the cold, but on heating a violent reaction takes place. By distilling the product, the anhydrous acid is obtained in the form of a colourless liquid, boiling at about 121° C., and having an odour greatly resembling sulphuretted acetic acid. On mixing with water it falls to the bottom, without, at first, suffering any change; on standing, however, it is slowly dissolved and decomposed into sulphuretted acetic acid and ordinary acetic acid. This change takes place much more rapidly on heating,

$$\left.\begin{matrix} C_2H_3O \\ C_2H_3O \end{matrix}\right\} S + \left.\begin{matrix} H \\ H \end{matrix}\right\} O = \left.\begin{matrix} C_2H_3O \\ H \end{matrix}\right\} S + \left.\begin{matrix} C_2H_3O \\ H \end{matrix}\right\} O.$$

It appears that anhydrous sulphuretted acetic acid is also produced by acting on the othyl-sulphide of lead with chloride of othyl, at all events chloride of lead is formed. Chloride of benzoyle gives with the lead salt a similar reaction, and it is probable that an intermediate sulphuretted acid is formed, having the formula

$$\left.\begin{matrix} C_2H_3O \\ C_7H_5O \end{matrix}\right\} S.$$

Thiacetate of Ethyl.—Sulphuretted Acetic Ether.—This compound may be prepared by the action of pentasulphide of phosphorus on acetic ether. It is a liquid lighter than water, and possesses an odour resembling acetic ether and sulphuretted hydrogen. It boils at about 80° C.

It will be seen that the action of tersulphide and pentasulphide of phosphorus above described produces sulphuretted organic compounds by substituting sulphur for oxygen. The compounds obtained in this way may also be formed by replacing one or two atoms of hydrogen in sulphuretted hydrogen (H_2S), or one or two atoms of metal in sulphide of potassium (K_2S), or in sulphide of hydrogen and potassium (KHS), by organic radicals. Mercaptan and the sulphides of alcohol radicals have, in fact, been long obtained in this manner.

The formation of a sulphuretted compound containing an acid radical has been observed by Gerhardt by acting on sulphide of lead with chloride of othyl. I have not made many experiments of this kind, but I have observed that chloride of benzoyle is not decomposed by sulphuretted hydrogen, while it (as well as chloride of othyl) gives a reaction with sulphide of hydrogen and potassium yielding chloride of potassium.

I am continuing these researches, and believe the above reactions will furnish many new compounds, and will tend to complete our knowledge of some of those organic and inorganic compounds now known.

The Society then adjourned to the 27th of April.

April 27, 1854.

The EARL of ROSSE, President, in the Chair.

Edward Joseph Cooper, Esq., was admitted into the Society.

In accordance with the notice given at the last Meeting of the Society, the Right Hon. Lord Ashburton was proposed for election and immediate ballot, to which, as a Peer of the Realm, his Lordship is entitled. The ballot having been taken, Lord Ashburton was declared duly elected.

The following papers were read:—

I. "On the Changes produced in the Blood by the administration of Cod-liver Oil and Cocoa-nut Oil." By THEOPHILUS THOMPSON, M.D., F.R.S. Received March 30, 1854.

The author has found that during the administration of cod-liver oil to phthisical patients their blood grew richer in red corpuscles, and he refers to a previous observation of Dr. Franz Simon to the same effect. The use of almond-oil and of olive-oil was not followed by any remedial effect, but from cocoa-nut oil results were obtained almost as decided as from the oil of the liver of the Cod, and the author believes it may turn out to be a useful substitute. The oil employed was a pure cocoa oleine, obtained by pressure from crude cocoa-nut oil, as expressed in Ceylon and the Malabar coast from the *Copperah* or dried cocoa-nut kernel, and refined by being treated with an alkali and then repeatedly washed with distilled water. It burns with a faint blue flame, showing a comparatively small proportion of carbon, and is undrying.

The analysis of the blood was conducted by Mr. Dugald Campbell. The whole quantity abstracted having been weighed, the coagulum was drained on bibulous paper for four or five hours, weighed and divided into two portions. One portion was weighed

and then dried in a water-oven, to determine the water. The other was macerated in cold water until it became colourless, then moderately dried and digested with ether and alcohol to remove fat, and finally dried completely and weighed as fibrin. From the respective weights of the fibrin and the dry clot that of the corpuscles was calculated. The following were the results observed in seven different individuals affected with phthisis in different stages of advancement:—

		Red corpuscles.	Fibrin.
First stage, before the use of cod-liver oil	Female	129·26	4·52
	Male	116·53	13·57
First stage, after the use of cod-liver oil	Female	136·47	5·00
	Male	141·53	4·70
Third stage, after the use of cod-liver oil	Male	138·74	2·23
Third stage, after the use of cocoa-nut oil......	Male	139·95	2·31
	Male	144·94	4·61

II. "On a property of Numbers." By the Rev. James Booth, LL.D., F.R.S. &c. Received April 6, 1854.

I know not whether the following property of numbers has been made public.

A number of six places, consisting of a repetition of a period of any three figures, is divisible by the prime numbers 7, 11 and 13. Thus 376376, 459459, 301301 are so divisible.

A number N of six places may be thus written:—

$$N=100.000\,a+10.000\,b+1000\,c+100\,d+10\,e+f,$$

which, when divided by 7, will give a quotient q and a remainder $5a+4b+6c+2d+3e+f$.

Now if $d=a$, $e=b$, $f=c$, this remainder may be written $7(a+b+c)$, which is divisible by 7, whatever be the values of a, b, c.

In like manner if a number of six places be divided by 13, the remainder will be

$4a+3b+12c+9d+10e+f$; and, as before, if $d=a$, $e=b$, $f=c$,

the remainder may be written $13(a+b+c)$, which is divisible by 13, whatever be the value of a, b and c.

In the same way it may be shown that a number of this kind is divisible by 11.

When the first figure of the period is 0, and the second any whatever i and j, the number is $0ij0ij=ij0ij$; or any number of five places, the first two and the last two being the same, while the middle place is 0, is divisible by 7, 11 and 13. Thus 34034, 14014 are so divisible.

When the first two places are 0, the number may be written $00i00i=i00i$, or any number of four places, the first and last figures being the same, while the two middle places are 0, is divisible by 7, 11 and 13. Thus 5005, 8008 are so divisible.

Like properties may be found for 17, 19, 23, but the periods are longer. The prime divisor being $2n+1$, it is manifest the number of places in the period cannot exceed, however it may fall short of n.

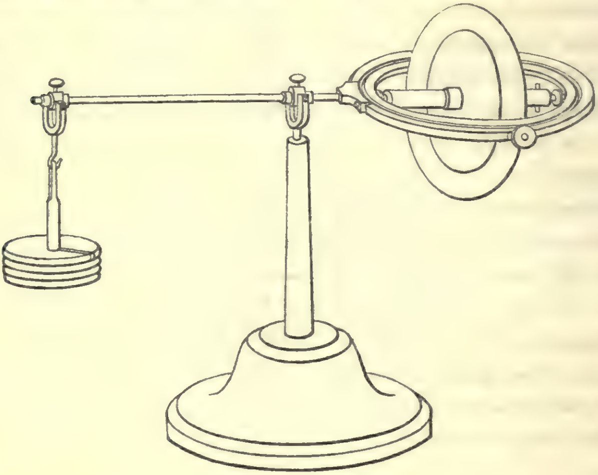
Thus when the divisor is 17, the number of places in the period is eight.

III. "On Fessel's Gyroscope." By C. Wheatstone, Esq., F.R.S. Received April 6, 1854.

Since the announcement of M. Foucault's beautiful experiment which has afforded us a new mechanical proof of the rotation of the earth on its axis, the phenomena of rotary motion have received renewed attention, and many ingenious instruments have been contrived to exhibit and to explain them. One of the most instructive of these is the Gyroscope invented by M. Fessel of Cologne, described in its earlier form in Poggendorff's Annalen for September 1853, and which, with some improvements by Prof. Plücker and some further modifications suggested by myself, I take the present opportunity of bringing before the Royal Society.

It is thus constructed: a beam is capable of moving freely round a horizontal axis which is itself moveable round a vertical axis, so that the beam may move in any direction round a fixed point; at one end of the beam is fixed a horizontal ring which carries a heavy

disc, the axis of rotation of which is in a line with the beam ; at the opposite extremity is a shifting weight by means of which the equilibrium of the beam may be established or disturbed at pleasure.



The Gyroscope.

If the beam be brought into equilibrium, and the disc be rapidly rotated, by means of a thread unrolled from its axis, it will be seen that the beam has no tendency to displace itself in any direction. Not so, however, if the equilibrium be in any way disturbed; on moving the weight towards the centre of the beam, thus causing the *disc* to preponderate, it will be observed that if the disc rotates from right to left the beam will move round the vertical axis also from right to left; and if the motion of the disc be reversed the rotation of the beam will be reversed also. On causing the *equipoise* to preponderate contrary effects will take place. The velocity of the rotation of the beam round the vertical axis increases in proportion to the disturbance of the equilibrium. It will also be observed that, notwithstanding the increased or diminished action of gravity on the disc, its axis of rotation always preserves the same inclination to the vertical axis at which it has been originally placed. The effect produced is a seeming paradox. When the equilibrium is disturbed while the disc is at rest, the beam being placed in any other position than the vertical, gravity acts so as to turn it round a horizontal axis; but when the

disc is in motion the usual effect of gravity disappears, and there is substituted for it a continued rotation round a vertical axis, that is, round an axis perpendicular to the plane which contains the axes of the two original rotations.

A similar composition of forces takes place when the disc is caused to rotate while the equilibrium of the beam is maintained, by impressing on the beam a rotation round the vertical axis. When the disc rotates from right to left, the slightest pressure tending to produce rotation round the vertical axis in the same direction, causes the end of the beam carrying the disc to ascend, and a pressure in the opposite direction causes it to descend, that is, the beam is constrained to move round a horizontal axis perpendicular to the vertical plane which contains the two axes of impressed rotation, a case exactly analogous to the preceding. The beam ascends and descends in like manner, after rotation has spontaneously taken place round the vertical axis in consequence of the equilibrium being disturbed, whenever this rotation is any how accelerated or retarded; the disc rotating from right to left and its weight predominating, the rotation round the vertical axis is from left to right; accelerating the latter motion will cause the disc to descend, and retarding it will occasion it to ascend.

As the centre of gravity of the beam is below its point of suspension, even when equipoised it is in perfect equilibrium only when it is horizontal, consequently, if it be elevated above or depressed below this position it will endeavour to resume it, tending to produce in the two cases rotations in opposite directions round a horizontal axis; the rotation of the disc combined with this tendency gives rise, as in the other cases I have mentioned, to a continued rotation round the vertical axis. If the disc rotate from right to left, and the end of the beam carrying it be elevated above the horizontal position, the rotation round the vertical axis will be from right to left; if, on the contrary, the same end of the beam be depressed below the horizontal position, that rotation will be from left to right.

In all the experiments above mentioned the axis of the rotating disc has remained in the prolongation of the beam, but, by means of an internal ring moveable round a line perpendicular thereto, this axis may be placed at any inclination and at any azimuth with respect to it. Very obvious considerations show that the

inclination of this axis should produce no difference in the character of the effects but merely in their intensity, since in any inclined position of the disc its rotation is resolvable into two others, one perpendicular to the beam, and the other, which is incapable of producing any effect, in a plane containing it. When the axis of the rotating disc is vertical and at right angles to the beam, no rotation on the vertical axis ought to take place in any case; but, contrary to this expectation, although the beam be horizontal and in perfect equilibrium, a motion round the vertical axis results, which is in opposite directions according as one or the other end of the axis of the disc is uppermost. It is, however, easy to see that this rotation is not owing to the same cause which gives rise to the phenomena hitherto considered, for whether it be accelerated or retarded no change is produced in the horizontal position of the beam; it is, in fact, occasioned by the friction of the pivots of rotation dragging the beam into a corresponding motion. Attention to this extraneous cause of rotation will explain numerous anomalies which present themselves in many of the instruments contrived to exemplify the phenomena of combined rotary motions. It is one of the advantages of Fessel's apparatus that the phenomena may be exhibited in their more important phases without being affected by this source of error.

We may form a clearer conception of these phenomena by first considering some simpler facts which do not appear to me to have been hitherto sufficiently attended to. For this purpose let the system of rings carrying the disc be removed from the rest of the apparatus, and by unfastening the tightening screw let the inner ring be allowed to move freely within the outer. Having set the disc in rapid rotation, hold the outer ring at the extremities of the diameter which is in the plane in which the axis of motion of the disc is free to move, then giving to the outer ring a tendency to rotation round that diameter, it will be observed that, in whatever position the axis is, it will fly to place itself in the fixed axis thus determined, and rotation will take place round it in the same direction. Considerable resistance is felt so long as the moveable axis is changing its position, but when once it coincides with the fixed axis the rotation of the external ring round its diameter is effected with facility. A slight alternate motion of the outer ring, tending to give to it rotations in opposite directions, will occasion a continued

rotation of the moveable axis. The same result takes place when an endeavour is made to rotate the outer ring round an axis perpendicular to its plane. In all cases when the axis of the rotating disc is free to move in a plane, and the outer ring is constrained to rotate round a line in this plane, the moveable axis will place itself so as to coincide with that line, and so that the disc shall rotate in the same direction as the ring; if the fixed axis be in a different plane the moveable axis will assume permanently that position in its plane which approaches nearest to the former. The moveable axis is thus apparently attracted towards the fixed axis if the rotations are in the same direction, and repelled from it if the rotations are in opposite directions.

In the experiments just described the free and constrained axes of rotation intersect, but in Fessel's apparatus they are distant from each other. In the latter case the rule must be thus modified, that the free axis of rotation tends to place itself *parallel* to the constrained axis of rotation, or to as near a position thereto as possible. By this principle all the results manifested are easily explained. The beam being in equilibrium, a motion impressed on it round the vertical axis causes it to ascend or descend, because the axis of the rotating disc tends to place itself parallel to the vertical axis of rotation and so that the disc rotates in the impressed direction. When the equilibrium of the beam is destroyed, gravity tends to make it rotate round a horizontal axis; the axis of the disc endeavours to place itself parallel with that axis, but both being unchangeably at right angles to each other, the tendency to place itself there gives rise to a continued rotation. Other results with this apparatus, to which I have not yet adverted, are similarly explained. Fix the outer ring horizontally and loosen the inner ring, keeping them both however in the same plane; then, on moving the beam round the vertical axis, the axis of the rotating disc will immediately fly to place itself parallel thereto, with rotation of the disc in the impressed direction. The rings being placed in the vertical plane, the same result will take place if the beam be moved in a vertical plane, *i. e.* round a horizontal axis.

The following additional experiments may be made with the rings detached from the apparatus. The results are necessary consequences of what has been previously explained:—

1. Suspend, by means of a string, the outer ring at the extremity

of a diameter perpendicular to the axis of the inner ring; and, having loosened the latter, place it at right angles to the former. On causing the disc to rotate, its axis will retain its original position; but if the slightest effort be made to turn the outer ring round the vertical line, the axis of the rotating disc will instantly fly into this position, and the disc will move in the same direction as that of the impressed rotation.

2. The horizontality of the loose inner ring being restored, if a weight be suspended from either end of the axis of the disc, that axis will, while it preserves its horizontal or any inclined position, revolve round the vertical line; the direction of the motion will change if either the weight be applied to the opposite end of the axis or the disc rotate in the opposite direction. If this rotation be arrested, gravity will immediately cause the weighted end of the axis to descend.

3. Clamp the rings together either in the same plane or at right angles to each other, and fasten a string, in the first case, at the extremity of a diameter coinciding with the axis of the inner ring, and in the latter case at the extremity of a diameter perpendicular thereto. Having set the disc spinning, if a rotation round the vertical line be given to the system the axis of the disc will ascend, carrying with it the disc and rings notwithstanding their weight, and, even when the impressed rotation has ceased to act, will continue to rotate in the same direction until the motion of the disc ceases.

In this note I have purposely avoided entering into the mathematical theory of the phenomena, my intention having been solely to describe the apparatus exhibited and to give an intelligible account of its effects. Those who wish to investigate the subject more profoundly, will find the best guide in the Astronomer Royal's essay on Precession and Nutation published in his Mathematical Tracts.

May 4, 1854.

Colonel SABINE, R.A., Treas. and V.P., in the Chair.

In accordance with the Statutes, the Chairman read the following list of Candidates recommended by the Council to the Society for election:—

George James Allman, M.D.
Edward William Brayley, Esq.
Alexander Bryson, M.D.
J. Lockhart Clarke, Esq.
Joseph Dickinson, M.D.
Ronald Campbell Gunn, Esq.
Robert Hunt, Esq.
John Bennet Lawes, Esq.
Robert Mallet, Esq.
Charles May, Esq.
Capt. Thomas E. L. Moore, R.N.
Captain Richard Strachey.
Robert Dundas Thomson, M.D.
Samuel Charles Whitbread, Esq.
William Crawford Williamson, Esq.

The following papers were read:—

I. "Account of Researches in Thermo-electricity." By Professor W. Thomson of Glasgow, F.R.S. Received April 20, 1854.

§ I. *On the Thermal Effects of Electric Currents in Unequally Heated Conductors.*

Theoretical considerations (communicated in December 1851 to the Royal Society of Edinburgh), founded on observations which had been made regarding the law of thermo-electric force in an unequally heated circuit of two metals, led me to the conclusion that an electric current must exercise a convective effect on heat in a homogeneous metallic conductor of which different parts are kept at different temperatures. A special application of the reasoning to the case of a compound circuit of copper and iron was made, and it is repeated here because of the illustration it affords of the mechanical principles on which the general reasoning is founded.

Becquerel discovered that if one junction of copper and iron, in a circuit of the two metals, be kept at an ordinary atmospheric temperature, while the other is raised gradually to a red or white heat, a current first sets from copper to iron through the hot junction, increasing in strength only as long as the temperature is below about 300° Cent.; and becoming feebler with farther elevations of temperature until it ceases, and a current actually sets in the contrary direction when a high red heat is attained. Many experimenters have professed themselves unable to verify this extraordinary discovery, but the description which M. Becquerel gives of his experiments leaves no room for the doubts which some have thrown upon his conclusion, and establishes the thermo-electric inversion between iron and copper, not as a singular case (extraordinary and unexpected as it appeared), but as a phenomenon to be looked for between any two metals, when tried through a sufficient range of temperature, especially any two which lie near one another in the thermo-electric series for ordinary temperatures. M. Regnault has verified M. Becquerel's conclusion so far, in finding that the strength of the current in a circuit of copper and iron wire did not increase sensibly for elevations of temperature above 240° Cent., and began to diminish when the temperature considerably exceeded this limit; but the actual inversion observed by M. Becquerel is required to show that the diminution of strength in the current is due to a real falling off in the electromotive force, and not to the increased resistance known to be produced by an elevation of temperature.

From Becquerel's discovery it follows that, for temperatures below a certain limit, which, for particular specimens of copper and iron wire, I have ascertained, by a mode of experimenting described below, to be 280° Cent., copper is on the negative side of iron in the thermo-electric series, and on the positive side for higher temperatures; and at the limiting temperature copper and iron are thermo-electrically neutral to one another. It follows, according to the general mechanical theory of thermo-electric currents referred to above, that electricity passing from copper to iron causes the absorption or the evolution of heat according as the temperature of the metals is below or above the neutral point; but neither evolution nor absorption of heat, if the temperature be precisely that of neutrality (a conclusion which I have already partially verified by

experiment). Hence, if in a circuit of copper and iron, one junction be kept about 280°, that is, at the neutral temperature, and the other at any lower temperature, a thermo-electric current will set from copper to iron through the hot, and from iron to copper through the cold junction; causing the evolution of heat at the latter, and the raising of weights too if it be employed to work an electro-magnetic engine, but not causing the absorption of any heat at the hot junction. Hence there must be an absorption of heat at some part or parts of the circuit consisting solely of one metal or of the other, to an amount equivalent to the heat evolved at the cold junction, together with the thermal value of any mechanical effects produced in other parts of the circuit. The locality of this absorption can only be where the temperatures of the single metals are non-uniform, since the thermal effect of a current in any homogeneous uniformly heated conductor is always an evolution of heat. Hence there must be on the whole an absorption of heat, caused by the current in passing from cold to hot in copper, and from hot to cold in iron. When a current is forced through the circuit against the thermo-electric force, the same reasoning establishes an evolution of heat to an amount equivalent to the sum of the heat that would be then taken in at the cold junction, and the value in heat of the energy spent by the agency (chemical or of any other kind) by which the electromotive force is applied. The aggregate reversible thermal effect, thus demonstrated to exist in the unequally heated portions of the two metals, might be produced in one of the metals alone, or (as appears more natural to suppose) it may be the sum or difference of effects experienced by the two. Adopting as a matter of form the latter supposition, without excluding the former possibility, we may assert that either there is absorption of heat by the current passing from hot to cold in the copper, and evolution, to a less extent, in the iron of the same circuit; or there is absorption of heat produced by the current from hot to cold in the iron, and evolution of heat to a less amount in the copper; or there must be absorption of heat in each metal, with the reverse effect in each case when the current is reversed. The reversible effect in a single metal of non-uniform temperature may be called a convection of heat; and to avoid circumlocution, I shall express it, that the vitreous electricity carries heat with it, or that the specific heat of vitreous electricity is positive,

when this convection is in the nominal "direction of the current," and I shall apply the same expressions to "resinous electricity" when the convection is against the nominal direction of the current. It is established then that one or other of the following three hypotheses must be true :—

Vitreous electricity carries heat with it in an unequally heated conductor whether of copper or iron; but more in copper than in iron.

Or Resinous electricity carries heat with it in an unequally heated conductor whether of copper or iron; but more in iron than in copper.

Or Vitreous electricity carries heat with it in an unequally heated conductor of copper, and Resinous electricity carries heat with it in an unequally heated conductor of iron.

Immediately after communicating this theory to the Royal Society of Edinburgh, I commenced trying to ascertain by experiment which of the three hypotheses is the truth, as Theory with only thermo-electric data could not decide between them. I had a slight bias in favour of the first rather than the second, in consequence of the positiveness which, after Franklin, we habitually attribute to the vitreous electricity, and a very strong feeling of the improbability of the third. With the able and persevering exertions of my assistant, Mr. McFarlane, applied to the construction of various forms of apparatus and to assist me in conducting experiments, the research has been carried on, with little intermission, for more than two years. Mr. Robert Davidson, Mr. Charles A. Smith, and other friends have also given much valuable assistance during the greater part of this time, in the different experimental investigations of which results are now laid before the Royal Society. Only nugatory results were obtained until recently from multiplied and varied experiments both on copper and iron conductors; but the theoretical anticipation was of such a nature that no want of experimental evidence could influence my conviction of its truth. About four months ago, by means of a new form of apparatus, I ascertained that *resinous electricity carries heat with it in an unequally heated iron conductor*. A similar equally sensitive arrangement showed no result for copper. The second hypothesis might then have been expected to hold; but to ascertain the truth with certainty I have

continued ever since, getting an experiment on copper nearly every week with more and more sensitive arrangements, and at last, in two experiments, I have made out with certainty, that *vitreous electricity carries heat with it in an unequally heated copper conductor.*

The third hypothesis is thus established: a most unexpected conclusion I am willing to confess.

I intend to continue the research, and I hope not only to ascertain the nature of the thermal effects in other metals, but to determine its amount in absolute measure in the most important cases, and to find how it varies, if at all, with the temperature; that is, to determine the character (positive or negative) and the value of the specific heat, varying or not with the temperature, of the unit of current electricity in various metals.

§ II. *On the Law of Thermo-electric Force in an unequally heated circuit of two Metals.*

A general relation between the specific heats of electricity in two different metals, and the law of thermo-electric force, in a circuit composed of them according to the temperatures of their junctions, was established in the communication to the Royal Society of Edinburgh referred to above, and was expressed by an equation* which may now be simplified by the thermometric assumption

$$t=\frac{J}{\mu};$$

(μ denoting Carnot's function, J Joule's equivalent, and t the temperature measured from an absolute zero, about $273\frac{1}{2}^{\circ}$ Cent. below the freezing-point,) since this assumption defines a system of *thermometry in absolute measure,* which the experimental researches recently made by Mr. Joule and myself establish as not differing sensibly from the scale of the air-thermometer between ordinary limits. The equation, when so modified, takes the following form:—

$$F=J\left\{\frac{\Theta_S}{S}(S-T)+\int_T^S \vartheta\left(1-\frac{T}{t}\right)dt\right\},$$

where ϑ denotes the excess of the specific heat of electricity in the metal through which the current goes from cold to hot above the specific heat of the same electricity in the other metal, at the tem-

* See Proceedings R.S.E. Dec. 1851, or Philosophical Magazine 1852.

perature t; F the thermo-electric force in the circuit when the two junctions are kept at the temperatures S and T respectively, of which the former is the higher; and Θ_S the amount of heat absorbed per unit of electricity crossing the hot junction. The following relation (similarly simplified in form) was also established:—

$$\vartheta=\frac{\Theta}{t}-\frac{d\Theta}{dt}.$$

These relations show how important it is towards the special object of determining the specific heats of electricity in metals, to investigate the law of electromotive force in various cases, and to determine the thermal effect of electricity in passing from one metal to another at various temperatures. Both of these objects of research are therefore included in the general investigation of the subject.

The only progress I have as yet made in the last-mentioned branch of the inquiry, has been to demonstrate experimentally that there is a cooling or heating effect produced by a current between copper and iron at an ordinary atmospheric temperature according as it passes from copper to iron or from iron to copper, in verification of a theoretical conclusion mentioned above: but I intend shortly to extend the verification of theory to a demonstration that reverse effects take place between those metals at a temperature above their neutral point of about 280° Cent.; and I hope also to be able to make determinations in absolute measure of the amount of the Peltier effect for a given strength of current between various pairs of metals.

With reference to laws of electromotive force in various cases, I have commenced by determining the order of several specimens of metals in the thermo-electric series, and have ascertained some very curious facts regarding varieties in this series which exist at different temperatures. In this I have only followed Becquerel's remarkable discovery, from which I had been led to the reasoning and experimental investigation regarding copper and iron described above. My way of experimenting has been to raise the temperature first of one junction as far as the circumstances admit, keeping the other cold, and then to raise the temperature of the other gradually, and watch the indications of a galvanometer during the whole process. When an inversion of the current is noticed, the changing temperature is

brought back till the galvanometer shows no current; and then (by a process quite analogous to that followed by Mr. Joule and Dr. Lyon Playfair in ascertaining the temperature at which water is of maximum density) the temperatures of the two junctions are approximated, the galvanometer always being kept as near zero as possible. When the difference between any two temperatures on each side of the neutral point which give no current is not very great, their arithmetical mean will be the neutral temperature. A regular deviation of the mean temperature from the true neutral temperature is to be looked for with wide ranges, and a determination of it would show the law according to which the difference of the specific heat of electricity in the two metals varies with the temperatures; but I have not even as yet ascertained with certainty the existence of such a deviation in any particular case. The following is a summary of the principal results I have already obtained in this department of the subject.

The metals tried being,—three platinum wires (P_1 the thickest, P_2 the thinnest, and P_3 one of intermediate thickness), brass wires (B), a lead wire (L′), slips of sheet lead (L), copper wires (C), and iron wire (I), I find that the specimens experimented on stand thermo-electrically at different temperatures in the order shown in the following Table, and explained in the heading by reference to bismuth and antimony, or to the terms "negative" and "positive" as often used :—

Temp. Cent.	Bismuth "Negative." ... Antimony "Positive."
−20	...P_3...*c* ......P_2 ....P_1......................I.........
0	...P_3...*l′* ......P_2 ......C ......P_1......................I.........
37	...P_3......*b*...$\underrightarrow{\{L'P_2\}}$...... C...$P_1$......................I.........
64	...P_3...........P_2... *b*... *l′* $\underrightarrow{\{CP_1\}}$ ....I.........
130	...P_3...........P_2...........$\underrightarrow{\{BP_1\}}$...L... C...... ...I.........
140	...P_3...........P_2.............. P_1...$\underrightarrow{\{BL\}}$ ......C ...I.........
280	...P_3...........P_2.............. P_1........... *b*... $\underrightarrow{\{CI\}}$ ......
300	...P_3...........P_2.............. P_1............... *b*......I......C

It must be added, by way of explanation, that the bracket enclosing the symbols of any two of the metallic specimens indicates that they are neutral to one another at the corresponding temperature, and the arrow-head below one of them shows the direction in which it is changing its place with reference to the other, in the series, as the temperature is raised. When there is any doubt as to a position as shown in the Table, the symbol of the metal is a small letter instead of a capital.

The rapidity with which copper changes its place among some of the other metals (the platinums and iron) is very remarkable. Brass also changes its place in the same direction possibly no less rapidly than copper; and lead changes its place also in the same direction but certainly less rapidly than brass, which after passing the thick platinum wire (P_1) at 130° Cent. passes the lead at 140°, the lead itself having probably passed the thick platinum at some temperature a little below 130°*.

The conclusion as regards specific heats of electricity in the different metals, from the equation expressing thermo-electric force given above, is that the specific heat of vitreous electricity is greater in each metal passing another from left to right in the series as the temperature rises than in the metal it passes: thus in particular,—

The specific heat of vitreous electricity is greater in copper than in platinum or in iron; greater in brass than in platinum or in lead; and greater in lead than in platinum.

It is probable enough from the results regarding iron and copper mentioned above, that the specific heat of vitreous electricity is positive in brass; very small positive, or else negative, in platinum, perhaps of about the same value as in iron. It will not be difficult to test these speculations either by direct experiment on the convective effects of electric currents in the different metals, or by comparative measurements of thermo-electric forces for various temperatures in circuits of the metals, and I trust to be able to do so before long.

§ III. *On Thermo-electricity in crystalline metals, and in metals in a state of mechanical strain.*

Having recently been occupied with an extension of the mechani-

* I have since found that it does pass the thick platinum, at the temperature 118°. [May 16, 1854.]

cal theory to the phenomena of thermo-electricity in crystalline metals, I have been led to experimental investigation on this branch of the subject. The difficulty of obtaining actual metallic crystals of considerable dimensions made it desirable to imitate crystalline structure in various ways. The analogies of the crystalline optical properties which have been observed in transparent solids, in a state of strain, and of the crystalline structure as regards magnetic induction which Dr. Tyndall's remarkable experiments show to be produced not only in bismuth but in wax, thick paste of flour, and "the pith of fresh rolls," by pressure, made it almost certain that pressure or tension on a mass of metal would give it the thermo-electric properties of a crystal. The only case which I have as yet had time to try, verifies this anticipation. I have found that copper wire stretched by a weight bears to similar copper wire unstretched, exactly the thermo-electric relation which Svanberg discovered in a bar cut equatorially from a crystal of bismuth or antimony compared with a bar cut axially from a crystal of the same metal. Thus I found that:—

If part of a circuit of copper wire be stretched by a considerable force and the remainder left in its natural condition, or stretched by a less force, and if either extremity of the stretched part be heated, *a current sets from the stretched to the unstretched part through the hot junction*: and if the wire be stretched and unstretched on the two sides of the heated part alternately, the current is reversed (as far as I have been able yet to test, instantaneously) with each change the tension.

I intend to make similar experiments on other metallic wires; also to try the effect of transverse as well as of longitudinal tension on slips of sheet metal *with their ends at different temperatures*, when placed longitudinally in an electric circuit; and the effects of oblique tension on slips of metal similarly placed in a circuit, but kept with their ends at the same temperature and their *lateral edges unequally heated*. I have no doubt of being able so to verify every thermo-electric characteristic of crystalline structure, in metals in a state of strain.

Glasgow College, March 30, 1854.

P.S. April 19, 1854.—I have today found by experiment that iron

wire when stretched by a considerable force bears a thermo-electric relation to unstretched iron wire, the opposite of that which I had previously discovered in the case of copper wire; and I have ascertained that when the wire is alternately stretched and unstretched on the two sides of a heated part the current is reversed along with the change of tension, always passing from the unstretched to the stretched part, through the hot locality.

I hope before the end of the present Session to have a complete account of all the experiments of which the results are stated above, ready to communicate to the Royal Society.

II. "An Introductory Memoir upon Quantics." By Arthur Cayley, Esq., F.R.S. Received April 20, 1854.

The subject of Quantics is defined as the entire subject of rational and integral functions, and of the equations and loci to which these give rise, but the memoir relates principally to the functions called quantics; a quantic being in fact a rational and integral function, homogeneous in regard to a set of facients $(x, y\,..)$, or more generally homogeneous in regard to each of several such sets separately. A quantic of the degrees $m, m'\,..$ in the sets $(x, y\,..)$ $(x', y'\,..)$ &c. is represented by a notation such as

$$(*)(x, y\,..)^m(x', y'\,..)^{m'}..),$$

where the mark $*$ is considered as indicative of the absolute generality of the quantic. The coefficients of the different terms of the quantic may be either mere numerical multiples of single letters or elements, such as $a, b, c\,..$, or else functions (in general rational and integral functions) of such elements; this explains the meaning of the expression the elements of a quantic. The theory leads to the discussion of the derivatives called covariants. Of these covariants a very general definition is given as follows, viz. considering the quantic $(*)(x, y, .)^m (x', y'\,..)^{m'}..)$, and selecting any two facients of the same set, *e. g.* the facients x, y, it is remarked that there is always an operation upon the elements tantamount as regards the quantic to the operation xd_y, viz. if we differentiate with respect to each element, multiply by proper functions of the elements and add, the

result will be that obtained by differentiating with d_y and multiplying by x. And if the operation upon the elements tantamount to xd_y is represented by $\{xd_y\}$, then writing down the series of operations

$$\{xd_y\}-xd_y, \ldots \{x'd'_y\}-x'd_{y'}, \ldots \&c.,$$

where x, y are considered as being successively replaced by every permutation of two different facients of the set $(x, y\ldots)$, x', y' by every permutation of two different facients of the set $(x', y'\ldots)$ &c., then it is clear that the quantic is reduced to zero by each of the operations of the entire system, but this property is not by any means confined to the quantic; and any function having the property in question, *i. e.* every function which is reduced to zero by each operation of the entire system, is said to be a covariant of the quantic. The definition is afterwards still further generalized, and its connection explained with the methods given, in the memoir 'On Linear Transformations,' Camb. and Dub. Math. Journal, Old Series, t. iv., and New Series, t. i., and the 'Mémoire sur les Hyperdéterminants,' Crelle, t. xxx., and some other theorems given in relation to the subject. The latter part of the memoir relates to the theory of the quantic $(*)(x, y)^m$, and to the number of and relations between the covariants, and as part of such theory to the beautiful law of reciprocity of MM. Sylvester and Hermite.

May 11, 1854.

The EARL of ROSSE, President, in the Chair.

The Right Hon. Lord Ashburton was admitted into the Society.

The following paper was read :—

"On the relation of the Angular Aperture of the Object-Glasses of Compound Microscopes to their penetrating power and to Oblique Light." By J. W. GRIFFITH, M.D., F.L.S. Communicated by ARTHUR HENFREY, Esq., F.R.S. Received April 29, 1854.

The explanation given by Dr. Goring and others of the advantage of increased angular aperture in microscopic objective-glasses appears to the author to be correct, as applied to the case of opake objects, and accordingly his remarks in the present communication have reference to transparent objects only.

It is known that delicate markings on a transparent object, such as the valve of a *Gyrosigma*, may be rendered more distinctly visible by using an object-glass of large aperture, by bringing the mirror to one side, and by placing a central stop in the object-glass or the condenser or in both; the increased distinctness produced in these several ways being due to the illumination of the object by oblique light. Experiment also shows that the degree of obliquity of the light requisite varies with the delicacy or fineness of the markings, being greater as these are more delicate; so that the finest markings require the most oblique light which can possibly be obtained to render them evident, and the angular aperture of the object-glass must necessarily be proportionately large, otherwise none of these oblique rays could enter it.

If the parts of an object which refract the light are large in proportion to the power of the object-glass and of irregular form, they

will refract a certain number of rays, so that these cannot enter the object-glass; hence certain parts will become dark, and will map out, as it were, in the image formed of the object, the structural peculiarities of the same. But if the parts are minute, of a curved form and approximatively symmetrical, they will act upon the light transmitted through them in the manner of lenses, and their luminous or dark appearance will vary according to the relation of the foci of these to that of the object-glass. Thus the parts of an object may appear dark and defined, from the refraction of the light out of the field of the microscope; also, from the concentration or dispersion of portions of the light by these parts, all the rays being admitted by the object-glass, or entering the field.

Another condition affecting distinctness consists in the relation which the luminousness or darkness of an object bears to that of the field or back ground upon which it is apparently situated.

The refraction of the light out of the field of the microscope or beyond the angle of aperture of the object-glass is the ordinary cause of the outlines of objects becoming visible; and in these cases, an increase of the angular aperture of the object-glass will impair their distinctness, because it will allow of the admission of those rays which would otherwise have been refracted from the field, and the margins will become more luminous and less contrasted with the luminous field.

The cause of the distinctness of an object by refraction when all or nearly all the rays enter the field of the microscope, may be investigated in a drop of oil immersed in water, or in a drop of milk, as illuminated by light reflected from an ordinary mirror. The refractive power of the globules is so great and their form such, that each acts as a minute spherical lens; and the parts within the margin will appear light or dark according to the relation of the focus of the little lens to that of the object-glass. Under an object-glass of small aperture and moderate power the outline will appear black, because the marginal rays do not enter the object-glass. If the object-glass be of sufficient aperture to admit these marginal rays, the black margin will disappear, and the little lens will only be distinguishable by the above focal relation. Its appearance under oblique light (thrown from all sides, as when the condenser and a central stop are used) will vary; but taking the case of extreme

obliquity of the rays, the lens will only be visible by a luminous margin from reflexion, giving it a very beautiful annular appearance. Hence it is more distinct by direct, or slightly oblique, than by very oblique light.

But in certain objects, the irregularities of structure are of such extreme minuteness, or the difference of the refractive power of the various portions of the structure is so slight, that the course of the rays is but little altered by refraction on passing through them, and, under ordinary illumination, all the rays will enter the object-glass; neither are the rays accumulated into little cones or parcels, of sufficient intensity to map out the little light or dark spots in the field of the microscope, according to the relation of their foci with that of the object-glass.

Let us take the instance of an object with minute depressions on the surface, as the valve of a *Gyrosigma*. These are so minute, that when the light reflected from the ordinary mirror is used, the rays passing through the depressed and the undepressed portions, are not sufficiently refracted to cause either set to be excluded from the object-glass, consequently both sets will enter it. The slightly oblique and converging rays passing through a portion of the valve become separated into two sets, one passing through the thinner depressed portions, the other through the thicker and undepressed portions: still both sets enter the object-glass. But on transmitting oblique light through the object, one set of the rays will be refracted so as not to enter the object-glass, whilst the other set will gain admission; thus the two parts, which have differently refracted the rays, will become distinct. If the markings were more delicate, or if the difference between the refractive power of the two portions of the valve were less, both sets would enter the object-glass. But on rendering the light still more oblique, one set would be again excluded by being refracted out of the field. Hence it is evident why the angular aperture of the object-glass must be larger as the markings are finer, or the difference between the refractive power of the two portions of tissue is less; because the obliquity of the light requisite will be very great to cause the exclusion of one set of the rays, and the other set will be too oblique to enter the object-glass unless it be of correspondingly large aperture. This is the explanation of the advantage of oblique light. It has

no peculiar power of rendering objects distinct, as has sometimes been believed, and the following experiment, supposed to show such peculiar power, is really to be explained on different grounds. A piece of net, or some similar texture, is placed behind a hole made in a window-shutter, and when thus viewed, the fibres are not well seen; but when the texture is moved on one side, they become very distinctly visible, and this has been erroneously attributed to the illumination by oblique light; whereas the increased distinctness in the lateral position is owing principally to the circumstance that the object is then viewed on a dark instead of a white ground as in the first instance; although it is also true that in this position the oblique rays, being reflected in large numbers from the fibres into the eye, contribute to the distinct vision of the object when viewed as it then is upon a dark ground.

The most difficult point has been to explain, how an object-glass of large angular aperture will render markings evident, which were not visible under an object-glass of smaller aperture; because it would naturally be imagined that the larger aperture would admit both sets of rays, one of which was excluded by the object-glass of smaller aperture. The difficulty vanishes when it is recollected that the additional rays admitted by the object-glass of larger aperture are more oblique; hence one set of these rays will be refracted from the field of the microscope, whilst the other set will enter the object-glass and will illuminate the more highly refractive parts of the object; thus the two kinds of differently refractive structure become distinctly separated, one appearing dark, the other luminous; in fact, by means of the additional rays admitted by the larger aperture we illuminate more highly one part of the object whilst the illumination of the other is not increased. In short, the object is illuminated, first, by rays corresponding to those admitted by an object-glass of small aperture; and, secondly, by the additional rays admitted by the object-glass of larger aperture. The first set not being sufficiently oblique, no part of them is refracted beyond the angular aperture of the object-glass; the second, being more oblique, are refracted out of the field by certain parts of the object and not by others, and thus contribute to render its different parts distinguishable by contrast of darkness and illumination. The first set of rays, by illuminating all parts of the object, tend to diminish this

contrast, and consequently do not add to but impair the discriminative power of the object-glass for the fine markings of transparent objects, and accordingly these are rendered more distinctly visible by intercepting the less oblique rays by means of a central stop.

It has been here assumed that the oblique light requisite for the display of the markings upon objects is separated into two sets of rays by refraction; but the author observes that it might be questioned whether they are not separated by reflexion. There can be no doubt that the latter is not generally the case; perhaps the most important reason which may be assigned for this is, the considerable comparative breadth of the luminous portions of the valve of the *Gyrosigma* for instance. On transmitting unilateral light obliquely through the valve of an *Isthmia*, in which the depressions are so large, in such manner that part of it is reflected by portions of them, it is easily seen how small the amount of reflected light is; and this because the surface of the depressions is curved, and thus the portions inclined at the requisite angle for reflexion are also very small. As the amount of light reflected is so small in this case, it would be inappreciable in that of the *Gyrosigma*, in which the depressions are so exceedingly minute. In fact, attention to this point affords a ready means of distinguishing whether an object is illuminated by reflexion or refraction.

The author next considers the relation of the *penetrating* power of an object-glass to its *defining* power. Penetrating power depends upon angular aperture, and consequently on oblique light. The question whether there be any essential difference between penetrating and defining power is best answered by experiment. If we take a fragment of the valve of an *Isthmia* and examine it under a high power of small aperture, all the parts are very distinctly seen by the ordinary light of the mirror; and the various depths of shadow of the different parts of the depressions and the undepressed portions render these also clearly distinguishable; and when an object-glass of very large aperture is used, the distinctness is rather impaired than improved. But if we examine a fragment of the valve of a *Gyrosigma*, and this requires an object-glass of large aperture to render the markings visible, no distinction of the various parts of the depressions and the undepressed portions is visible; all we see is, that the depressions as a whole are dark and the undepressed por-

tions are luminous. Hence the *Isthmia* requires defining power, whilst the *Gyrosigma* requires penetrating power or large angle of aperture to exhibit the markings; yet the structures differ only in size. And there can be no doubt that if we could examine the valve of the *Gyrosigma* under a power as high relatively to the size of the depressions, as that under which we can examine the *Isthmia*, the same relations being preserved between the angle of aperture of the object-glass and the angular inclination of the refracted rays, the various parts of the depressed and undepressed portions would be equally recognizable in both cases.

This is also true of fine lines scratched or etched on glass; for although the coarser lines upon glass micrometers are well seen with an object-glass of small aperture with good defining power and direct light, yet the finest lines upon Nobert's test-slide require penetrating power in the object-glass, and oblique light. Large angular aperture or *penetrating* power is but a very imperfect substitute for *defining* power—an important point which the author believes has not hitherto been noticed, and to which he would invite the earnest attention of object-glass makers.

The author concludes by observing that his remarks have been principally confined to one class of objects requiring penetrating power, viz. the valves of the Diatomaceæ. This has been done advisedly, because the scales of insects, which may be regarded as forming the type of the other class, involve considerations of a mixed kind, which would have tended to confuse the subject. The longitudinal ridges upon the scales of insects, in their relation to penetration, may be viewed as representing the undepressed portions of the valves of the Diatomaceæ; and the same explanation will apply to the visibility of both under various conditions. The transverse lines seen upon the scales are not indications of true structure; but their origin, as also that of the lines seen upon the valves of the Diatomaceæ, from circular or angular depressions, does not come within the conditions involved in the principle which it has been the object here to elucidate. It will suffice to say that the true structures producing the appearance of transverse markings upon the scales of insects are best resolved by small angular aperture and good definition.

It has been assumed also, that the markings upon the valves of

the Diatomaceæ arise from depressions. This can be proved to be the case in the larger ones (*Isthmia*, &c.); and there is sufficient evidence to render it at least highly probable in the remainder. But this is an unessential point as regards the principle, and therefore it has not been dwelt upon.

May 18, 1854.

The EARL of ROSSE, President, in the Chair.

The following gentlemen were recommended by the Council for election as Foreign Members:—

Karl Ernst von Baer.
Michel Chasles.
Friedrich Wöhler.

The following paper was read:—

"On some conclusions derived from the Observations of the Magnetic Declination at the Observatory of St. Helena." By Colonel EDWARD SABINE, R.A., V.P.R.S. Received May 18, 1854.

The author commences with the following preliminary remarks:—

"The part taken by the Royal Society in promoting, by its influence with Government, the establishment of the Colonial Magnetic Observatories, and in drawing up instructions for the guidance of those who were employed in them, makes it the duty of the person charged with their superintendence, to spare no pains to place before the Fellows, on suitable occasions, the results of researches designed to obtain a foundation of facts, on which a correct theory of the magnetic variations might be framed, and an insight be gained into the nature of the physical agency by which they are produced.

"In this first stage of scientific inquiry, when we have only the phenomena themselves to guide us in their classification, or to indicate by apparent correspondences the existence of some causal connexion of which we have no other knowledge than that which the observations themselves may afford, the first difficulty to be met consists, in disentangling from the complication in which the mag-

netic variations proceeding from different causes first present themselves, the effects which may appear to be due to certain amongst them; and in presenting these in some methodical order or arrangement, which may best assist the physicist or the mathematician in his conception of the problem or problems, to the solution of which he may desire to apply himself.

"The first and most obvious separation of the magnetic variations is into those which are presented at one time at different parts of the earth's surface, and have special reference therefore to space; and those which present themselves at different times at one and the same place, and have special reference therefore to time. It is the object of magnetic surveys to collect the facts of the first, and of magnetic observatories the facts of the second, of these primary divisions. The present communication belongs to the second, and regards the variations depending upon time at a single station (St. Helena).

"Still, however, the phenomena even at a single station are too complicated for ready comprehension, and stand in need of further subdivision. This is most satisfactorily effected by the customary separation into three classes, or *elements* as they are frequently termed, the Declination, the Inclination, and the Intensity of the Directive Force. The discussion is limited on the present occasion to a single element, the Declination, and to a portion only of the results obtained by the observations of that element at St. Helena."

After premising a description of the instrument with which the observations were made, and of the mode of observing and of recording the observations, which is omitted here because it may be found in the Introduction to the first volume of the 'St. Helena Magnetical Observations,' the author proceeds to the conclusions which he desires to notice, and to the manner in which these have been obtained, which we follow, by adopting, as nearly as may be convenient, his own words.

"Before we attempt to examine those periodical variations, or fluctuations about a mean value, which, from their having for periods, for example, the solar year or the solar day, we naturally refer to causes depending in some way upon the earth's place in its orbit relatively to the sun, or to the earth's revolution round its axis, it is desirable to examine, and if practicable, to eliminate the effects of a

variation which we have reason to believe belongs intrinsically to the magnetism of the earth itself. The geographical aspect, if we may so express it, of the terrestrial magnetism, or the different measure in which the magnetic force exists at different parts of the earth's surface, and the different directions which a magnet assumes in different places by virtue of this force, so far from being permanent, are found to be subject to a continual change, which differs from all other magnetic variations with which we are acquainted, inasmuch as it does not present to us the character of an oscillation of the phenomena around a mean value in periods of greater or less duration, but appears, especially when viewed generally in its operation over the whole globe, as a continuously progressive change; it has for this reason received the appropriate name of '*secular change*.' It is possible indeed that the magnetism of the earth may have its periods,—that the phenomena existing at one and the same epoch over the whole surface of the globe may be identically reproduced at a subsequent epoch,—and that what has been called the secular change of each of the magnetic elements, which we perceive to be in progress at any particular point of the surface, St. Helena for example, may be part of a succession of changes which operate in a cycle, of which the duration, vast as it may be, may hereafter be found to be calculable. But as far as our knowledge has yet gone, it is insufficient to justify the assumption of even approximate periodical laws of this variation of the terrestrial magnetism; and we must continue to regard it therefore for the present as a secular change, of which the period, if there be one, or the periods, if there be more than one, are as yet unknown. But although the secular change has no intrinsic relation, as far as we have been able to discover, to any of the periods of time determined by other phenomena, either of our own planet or of any other of the heavenly bodies, it is obvious that we may assign the average rate at which the change is taking place, in any of the magnetic elements and at any particular station (the declination for example at St. Helena), corresponding to any definite measure of time in usage amongst us (say for example a month, or the twelfth part of a solar year), by taking the successive differences between the monthly means of all the hourly observations in the first and second months of their continuance, then between the second and third months, then between the third and fourth, and

so on. By thus proceeding in the case of the Declination at St. Helena, we have sixty differences thus accruing in the five years of hourly observation, by which we find that the monthly increase of West Declination during these five years amounted on the average to $0' \cdot 657$, or to an annual increase of $7' \cdot 88$.

"It is not however necessary for this investigation that the system of observation should be *hourly*: a much less onerous system is sufficient, provided that the observations be distributed equably through the year, and that the intervals between the observations of each day be, approximately at least, equidistant. Before the commencement of the hourly series there had been fifteen months of two-hourly observations, and after its close the observations were continued for twenty-one months more at five hours of each day, the hours being such as to give by their combination a true mean value for each day. We are thus enabled to take in a more extended period, amounting to ninety-six consecutive months, or eight years, from which to derive the average rate of secular change at St. Helena. Proceeding as before, we find for this period an average rate of $0' \cdot 661$ for the increase of West Declination in a month, or an annual increase of $7' \cdot 93$ in a solar year. During these eight years the horizontal magnetic direction at St. Helena had consequently changed altogether rather more than one degree.

"When the number of years are few from which an annual average rate of secular change is derived, it is necessary to be particular in regard to the regular distribution of the observations as to months and hours, because observations made at one time of the year or at one hour of the day, are not strictly comparable with those made at other times of the year or at other hours of the day, unless indeed corrections based on a long series of observations at the same spot or in its vicinity are applied for the annual and diurnal variations. But when the periods of comparison include intervals of considerable length, the comparative influence of the annual and diurnal variations is greatly diminished, and, if the comparison extend over a *great* number of years, it may practically be disregarded. Now, St. Helena being a naval station, and frequently visited by navigators of our own and other countries, who have had the requisite knowledge and have been at the pains to take the necessary precautions to make trustworthy observations, we are able to collect from the nar-

ratives of their voyages a succession of determinations of the Declination, all made at the same spot, namely, at the one anchorage at St. Helena, which extend over a period of 236 years, or from 1610 to 1846. The following Table contains eleven such determinations, all from authorities of high repute, which are fortunately so far equably distributed in respect to the years when they were made, as to throw light not only upon the average amount of the secular change of declination during that long period, but also in a considerable degree upon the regularity, or uniformity with which the change has taken place. By treating these eleven determinations according to well-known methods, we obtain 11° 48′ as the west declination corresponding to the middle epoch, the year 1763, and 8′·05 as the most probable rate of the annual increase during the 236 years.

Declinations observed at the Anchorage at St. Helena.

1610. Davis	− 7° 13′	Calculated	− 8° 44′	Obs.—Calcul.	+ 1° 31′
1677. Halley	− 0 40	„	+ 0 16	„	− 0 56
1691. Halley	+ 1 00	„	+ 2 08	„	− 1 08
1724. Mathews	+ 7 30	„	+ 6 34	„	+ 0 56
1775. Wales	+ 12 18	„	+ 13 25	„	− 1 07
1789. Hunter	+ 15 30	„	+ 15 18	„	+ 0 12
1796. Macdonald	+ 15 48	„	+ 16 14	„	− 0 26
1806. Krusenstern	+ 17 18	„	+ 17 34	„	− 0 16
1839. Du Petit-Thouars	+ 22 17	„	+ 22 00	„	+ 0 17
1840. Ross	+ 22 53	„	+ 22 08	„	+ 0 45
1846. Bérard	+ 23 11	„	+ 22 57	„	+ 0 14

Mean Epoch 1763

Mean Declination +11° 48′

Annual Increase of West Declination 8′·05

"We have here then a striking example of the magnitude and character of the changes wrought at a particular station by this very remarkable feature of the earth's magnetic force. In less than two centuries and a half, the horizontal direction which a magnet takes at St. Helena by virtue of the terrestrial magnetic force has been found to have changed more than 30°, or more than a twelfth part of the whole circle: and when we further examine the facts more closely, we find reason to conclude that this great change has taken place by a steady, equable and uniform progression throughout the whole period. The rate of annual change derived from the eight years during which the observations were maintained by the detachment

of the Royal Artillery stationed at the Observatory ($7'{\cdot}93$) differs so slightly from that derived from the observations made at the anchorage from the earliest period at which observations are recorded (*i. e.* $8'{\cdot}05$), that we may practically regard them as the same. To examine whether this has been a uniform rate throughout the 236 years, or otherwise, the same calculation which gives $8'{\cdot}05$ as the most probable *average* rate of change between 1610 and 1846, will give also for each of the years in which the Declination was observed the most probable values of the Declination corresponding to the same rate of change supposed uniform. These calculated values are placed in the Table opposite to the years to which each belongs, and adjoining the observed values. The differences are shown in the next column. On inspecting these, we perceive that not one of the differences exceeds the limits, which, with a due consideration of the irregularities to which magnetic observations made on board ship are liable, may be ascribed to accidents of observation; and, what is still more important, that they fall indiscriminately to the east and to the west of the values calculated on the supposition of a uniform rate, and without the slightest appearance of any systematic character which might indicate that the rate had been otherwise than regular. We have reason to conclude, therefore, that, from the earliest date to which we can refer, the progression of secular change at St. Helena has gone on from year to year, as nearly as may be, in one uniform *annual* rate.

"The instruction to be derived from the St. Helena observations does not however stop here. By a suitable arrangement of the observations of the eight years, they may be made to show that, when allowance is made for comparatively very small irregularities superimposed upon the regular march of the phenomenon by disturbing causes which will be treated of in the sequel, the average annual change takes place by *equal aliquot portions in each month of the year*. The eight years of observation commenced with June 1841: if we take a mean of the eight monthly means in the eight Junes from 1841 to 1848, we shall have a better assured mean value of the Declination corresponding to the month of June, than if we had confined ourselves to a single year. If we then do the same with the eight Julys, and with each of the other months in succession, we shall have twelve monthly values for a year commencing

with June and ending with May, which will represent in a simple and condensed form the means of the whole eight years. These are exhibited in the next table, and we perceive at the first view that the increase of west declination is progressive in each month of the year without a single exception. If we desire to examine further the degree of approximation which these values present to a progression absolutely uniform, we may apply an aliquot portion of the annual value ($7'·93$) to each of the monthly means corresponding to the difference in time from the mean epoch (December 1). These aliquot portions are shown in the second column, and it will be seen by the third column, containing the mean declinations of the year deduced severally from the observation-values in the different months, with the correction for secular change assumed uniform applied, how very nearly the results derived from the several months approximate to one and the same value. The small differences which are shown in the last column are for the most part such as would probably disappear by a longer continuance of the observations; but we may notice, by the character of the signs, that there is also visible amongst them the indication of a comparatively very small semi-annual affection, depending on the sun's position on either side of the equator, which will be reverted to when treating of superimposed effects.

Months.	Mean Declination.	Correction for secular change to Dec. 1.	Mean Declination in the year.	Differences ($\psi-\psi'$).
June	23° 23'·42	+3'·64	23° 27'·06 = ψ'	+0'·22
July	23 24·45	+2·97	23 27·42 = ψ'	−0·14
August	23 24·91	+2·31	23 27·22 = ψ'	+0·06
September	23 25·30	+1·65	23 26·95 = ψ'	+0·33
October	23 26·32	+0·99	23 27·31 = ψ'	−0·03
November	23 27·07	+0·33	23 27·40 = ψ'	−0·12
December	23 27·73	−0·33	23 27·40 = ψ'	−0·12
January	23 28·29	−0·99	23 27·30 = ψ'	−0·02
February	23 29·23	−1·65	23 27·58 = ψ'	−0·30
March	23 29·76	−2·31	23 27·45 = ψ'	−0·17
April	23 30·21	−2·97	23 27·24 = ψ'	+0·04
May	23 30·69	−3·64	23 27·05 = ψ'	+0·23
Mean, corresponding to Dec. 1	23 27·28		23 27·28 = ψ	

"The same features of regularity and uniformity are manifested if the examination be further pursued into shorter periods, by comparing with each other the twenty-six *fortnightly* means in the year; but

enough has been already stated to show the magnitude, the regularity, and the systematic character of the changes called secular, which are thus produced by forces in constant operation at the surface of our planet. In our entire inability to connect these changes with any other of the phenomena of nature, either cosmical or terrestrial, we appear to have no other alternative than to view them as a constituent feature of the terrestrial magnetic force itself, and as one of its most remarkable characteristics, not to be overlooked by those who would seek to explain the phenomena of that force by means of a physical theory. The attempts which have sometimes been made to explain them by a supposed connection of the terrestrial magnetic phenomena with the distribution of land and sea at the surface of the globe, or with the distribution of heat on that surface, or by electrical currents excited by the rotation of the earth on its axis, contain no provision to meet a systematic variation of this nature; and break down altogether when the facts of the secular change are duly apprehended. From the phenomena of a single element at a single station, as here presented, we may assure ourselves that effects proceeding with so much order and regularity, which we cannot ascribe to any other cause than that of the terrestrial magnetism itself, and cannot therefore separate from its other manifestations, must find a place in any physical theory which professes to explain the phenomena of the earth's magnetism. To learn the changes in this and in the other magnetic elements which are simultaneously in progress in other parts of the globe, and to apprehend their mutual connexion and the general system of secular change which they indicate, it is necessary that the facts should be collected in the same manner as at St. Helena, at a great number of stations distributed over the earth's surface, and that they should be studied both separately and together. This may indeed appear a work of labour; but it is the most certain, if not the only certain mode of arriving at a correct knowledge of phenomenal laws, when the laws of their causation are wholly unknown. In this, as in similar studies, however complex the phenomena may appear at the first aspect,—and it is fully admitted that those of the secular magnetic change do appear extremely complex at the first view,—the mind soon begins to recognize order amidst apparent irregularity, and system amidst incessant variation. The order and regularity with

which we are impressed at a single station are soon perceived to characterize, in an equally remarkable manner, a general systematic change taking place connectedly over the whole surface of the globe, and which can everywhere be traced to have been continuously in operation since the earliest epoch of magnetic observation. To those who find pleasure in tracing phenomena of great apparent complexity to laws of comparative simplicity which appear to embrace them all, this study affords its own repayment; and it is indispensable towards the acquisition of a knowledge of the laws of terrestrial magnetism. By a comparison of the isogonic lines corresponding to different epochs (lines of equal Magnetic Declination employed by Halley and since found so useful in generalisation in this branch of the magnetic phenomena), we perceive that a secular change of the Declination, almost identical with that at St. Helena, has prevailed at the same time over the greater part of the southern Atlantic; and that from the *form* of the isogonic lines in that quarter of the globe (which has undergone very little variation in the last 200 years), the regularity of the progression, and its persistence in the same direction, is in accordance with that general progressive motion from east to west, which magneticians have long since recognized as distinguishing the general systematic change in the southern hemisphere from that in the northern, which takes place in the opposite direction; whilst from the form of the isogonic lines in that quarter, we may further anticipate that, at St. Helena, the secular change of the Declination will continue to take place in the same direction as at present, until the line drawn through the conical summits of the isogonic curves shall in its western progress pass the geographical meridian of that station."

The author then proceeds to the Variations which are found to take place in periods corresponding to a solar year and a solar day; a correspondence which, he remarks, "enables us to recognize a physical connexion, although we are still uncertain as to the mode of operation between cause and effect. A correct knowledge of the phenomena themselves is the surest guide to a correct judgement amongst the many theories which have been propounded in anticipation of that knowledge; and I have therefore taken this opportunity of bringing before the Society a careful analysis of the primary annual and diurnal variations at St. Helena attributable to solar influ-

ence, in the belief that they will be found to place in a very distinct light some points which are important to be kept in view in framing or in judging of such theories." For this purpose diagrams were exhibited, representing on a large scale the mean diurnal variation of the Declination at St. Helena in the different months of the year, and the annual variation at each of the twenty-four hours, both derived from the mean of five years of hourly observation; the secular change having been previously eliminated, these diagrams were regarded by the author as exhibiting what might be considered as typical views of the annual and diurnal variations, correct in their relations to the mean Declination in the year, or to the arithmetical mean of all the hourly observations in the year, taken as zero. As on the first aspect the diurnal phenomena in the several months are seen to separate themselves into two groups, having the equinoxes as at least approximate epochs of separation, the months in which the sun is north of the equator were coloured red, and those in which he is south of the equator were coloured blue.

Having in these diagrams the conjoint representation of two distinct classes of phenomena, a diurnal variation in each of the months, and an annual variation at each of the hours, the author proceeded to treat of each of these variations separately, commencing with the annual, which he illustrated by taking the hour of 7 A.M. as an example, and (referring to the diagram) showing the order and succession of the several months in the annual cycle at that hour, which are as follows:—in April the mean declination is about half a minute east of the mean declination in the year; in May about $2'$ east; in June about $2'\frac{1}{2}$ east; in July and August, when the sequence is slightly irregular, respectively $2'\cdot 1$ and $2'\cdot 6$ east; in September the declination is again approaching the mean line, being less than $1'\frac{1}{2}$ east of it; in October it has passed the mean line, being about $1'\frac{3}{4}$ *west* of it; November, December, January and February are congregated near the western extremity of the annual range, whilst in March we perceive that the declination is again approaching the mean line, and in April it has passed to the east of the mean line. "We have here, then," the author proceeds, "in the successive changes of the declination in the course of the year, the general fact of the existence of an annual variation, of which, at the solar hour of 7 A.M., selected as an example, or when the sun is five hours east

of the meridian, the phenomena are such as have been thus cursorily described. Were there no annual variation at that hour the different months would all have the same mean declination, and the extended figure, which in the diagram represents the annual cycle, would be concentrated into one point. The annual variation differs considerably at the different hours; but it is a general feature amongst them that the months on either side of the one solstice are either congregated together towards one extremity of the annual range at the hour, whilst the months on either side of the opposite solstice are similarly congregated at the opposite extremity, or the months of both solstices are contemporaneously in pretty rapid transition from the one extremity to the other. It is this *annual* variation which has been overlooked in the supposition entertained by a very eminent authority, that in the vicinity of the equator the magnetic direction would be found to be constant at all hours of the day and night. If we group together the monthly means of each period of six months separated by the equinoxes, we have two semiannual mean lines, each differing comparatively very slightly from any one of the months of which it is composed, but the two differing very greatly from each other, and both differing very considerably from the mean diurnal march in the year. If the latter line, viz. *the mean diurnal march in the year*, be projected as a straight line, as is done in the zero-line of fig. 1 in the annexed woodcut, the semiannual groups take respectively the forms exhibited in that figure, the continuous line being the semiannual march in the half year when the sun is north of the equator, and the dotted line the semiannual march when the sun is south of the equator. It is in this form that the phenomena of the annual variation in different parts of the globe may be most advantageously compared with each other. Fig. 2 represents the analogous phenomena at Toronto in 43° north, and fig. 3 those at Hobarton in 43° south latitude. The semiannual groups at Toronto and Hobarton have been obtained in precisely the same manner as those at St. Helena; the scale is the same in the three figures, *i. e.* ·5 of an inch to 1′·0 of Declination, the dotted and continuous lines refer respectively to the same periods of the year, and the zero line is in each figure the mean diurnal variation in the year at the station.

"In viewing these three figures, it is scarcely possible to doubt that

ILLUSTRATIONS OF THE ANNUAL VARIATION OF THE MAGNETIC DECLINATION.

Black line.—Mean Semiannual Diurnal Variation, March 22 to September 20.
Dotted line.—Mean Semiannual Diurnal Variation, September 22 to March 20.

Fig. 1.—St. Helena.

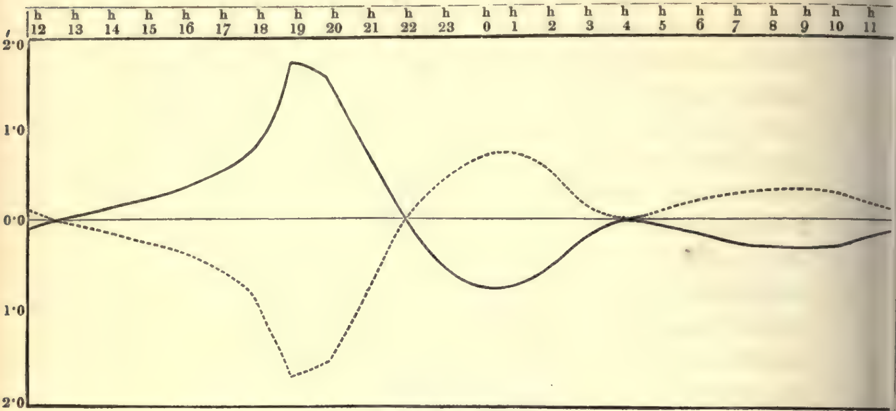


Fig. 2.—Toronto.

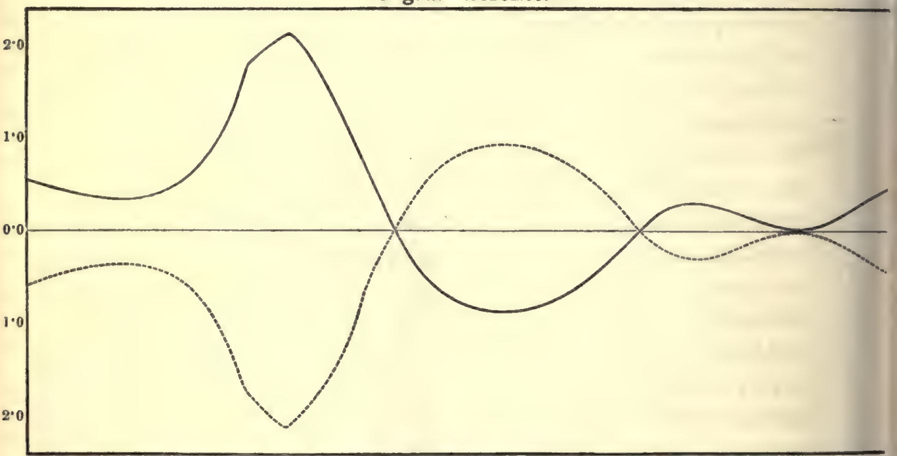
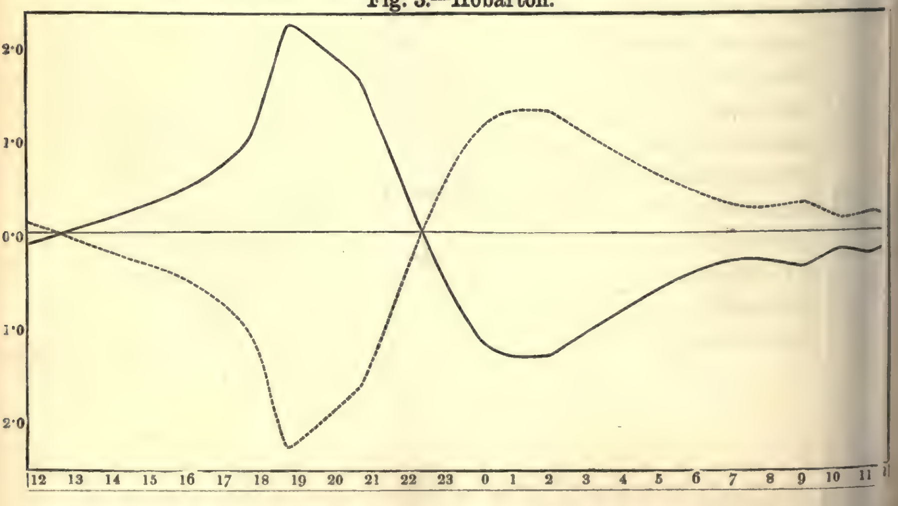


Fig. 3.—Hobarton.



they represent substantially the same phenomenon. The magnitude and inflexions of the curves are not indeed identical, but they approach so near to it that we may well suppose the small differences to be very minor modifications which will some day receive their explanation, It will be remarked that during the hours when the sun is above the horizon and the effects are greatest, the correspondence of the phenomena at the three stations is most striking, and that *there is no inversion of the phenomena in the opposite hemispheres*; in both (as well as at St. Helena, in the tropics), the Declination is easterly of the mean in the forenoon and westerly in the afternoon when the sun is north of the equator, and the reverse when the sun is south of the equator. The effects are the same at the three stations, though in the one hemisphere the sun being north of the equator corresponds to summer, and in the other hemisphere to winter; whilst in the tropics this distinction of seasons almost ceases to be sensible, and the epochs of maximum and minimum of temperature do not correspond with either of those of the extra-tropical stations. The phenomena thus represented embrace above 86° of latitude, presenting not only almost extreme contemporaneous diversities of climate, but also not less remarkable diversities of absolute dip, declination and magnetic force.

"No doubt can, I apprehend, be entertained that the annual variation which is here represented, is attributable, primarily, to the earth's revolution round the sun in a period of the same duration and in an orbit inclined to the equator. But in what way, it may be asked, does the sun superimpose upon the earth's magnetism this comparatively small but systematic magnetic variation? The similarity of effect, amounting almost indeed to identity at the hours when the sun is above the horizon of the station, taking place at stations where both the climatic and the terrestrial magnetic conditions are so dissimilar, seems to remove it altogether from those physical connexions, which have so often and in so many various ways been referred to as affording possible explanations of the magnetic variations. In this difficulty some assistance may perhaps be afforded by examining more closely, by means of the St. Helena observations, the epochs when the phenomena of one of the semiannual groups passes into the very dissimilar phenomena of the other semiannual group. This has been stated to take place

approximately at the equinoxes. The approximation, particularly at the September equinox, is very distinctly and definitely marked. The day of the equinox is the 21st of September; if a mean be taken of the diurnal march in the three weeks from the 1st to the 21st of September, the line which represents it scarcely differs sensibly at any hour of the twenty-four from the mean line of the preceding half-year, taken from the 22nd of March to the 20th of September; thus showing that the phenomena of that semiannual group are unchanged up to the time of the equinox. If in the same way a mean be taken of the diurnal march in the three weeks following the 21st of September, the line which represents them shows that the passage from the phenomena of one semiannual group to those of the other has not only commenced, but that in half the period of three weeks, *i. e.* within eleven days of the equinox, the change has already advanced very far towards its completion; and by the middle of October it is found to be quite complete, the mean in October retaining no trace of those semiannual characters which had undergone no modification ten days before the equinox." The facts thus stated were illustrated by diagrams.

"At the March equinox the commencement of the change is equally definite: no trace of change can be discovered in the mean from the 1st to the 20th of March, when compared with the mean of the six months from the 22nd of September to the 20th of March; the change then commences, but from some cause not yet apparent, the conversion from the phenomena of the one half-year to those of the other is effected less rapidly at this than at the September equinox. The mean of the month of April retains the distinct traces of the group which it has quitted, and is in fact a month of transition between the two groups, but in May the conversion is quite complete; the phenomena of that month have no characteristic distinguishable from those of June, July and August.

"From what has been stated in the preceding paragraphs, it will be evident that the epochs of the sun's passage of the equator have a very marked influence on the phenomena under consideration, and that the influence is the same and produces similar effects whether the station itself be north or south of the equator, and however diverse may be its climatic or magnetic conditions. The semiannual characteristics continue unchanged up to the days of the respective

equinoxes; these form the epochs when the transition from the characters of the one semiannual group to those of the other commences, the transition being completed a very few days after the September equinox, but somewhat less rapidly after the March equinox. Like the changes in the induced magnetism of ships, which follow immediately the changes in the terrestrial magnetism corresponding to the ship's altered geographical position, but *complete the change only after intervals of time of greater or less duration,* so the changes which we are here considering appear to commence at the equinoxial epochs, but to require a greater or less interval of time for their completion."

The divergence of the semiannual groups at the different hours from a mean march in the year has been shown in figs. 1, 2 and 3 by their comparison with the latter projected as straight lines, because the accordance of the divergence at the three stations is seen thereby in its simplest form. In another diagram the lines thus projected as straight lines were exhibited in their true Declination values, and compared with a Zero-line representing at each station the mean Declination in the year. "In the previous comparison of the *annual* variations at the three stations with each other, it was shown that there is no *inversion,* or *contrariety,* between the phenomena at Toronto and Hobarton as representatives of opposite hemispheres, the same semiannual group diverging (during the hours of the day when the characters are most marked) in the same direction at the same hours at both stations. But markedly opposite characteristics are shown when we compare the divergences of the *mean diurnal* variation in the year from the zero-line at different stations; these divergences, so far from according with each other at the two stations, present a strong contrast throughout; the divergence at Toronto being to the east at the hours when at Hobarton it is to the west, and *vice versâ.* St. Helena, moreover, which agrees with both the other stations in the divergences of the semiannual groups, differs from both in those of the mean of the whole year. The phenomena of the solar *annual* variation superimposed upon those of the solar *diurnal* variation,—and those of the solar *diurnal* variation itself,—are in this respect contradistinguished by important differences.

"To have completed the view of the solar variations of the Declination at St. Helena would have required a notice of the so-called irregular disturbances of that element, which are now known to have

a periodical character dependent on solar hours; and also of the remarkable cycle which is found to pervade all the magnetic variations depending upon the sun, corresponding in its period and epochs with those of the phenomena of the solar spots; but as both these subjects have been recently brought before the Society in separate memoirs, the author does not think it necessary to do more than merely advert to them on the present occasion."

June 1, 1854.

The Annual General Meeting for the election of Fellows was held this day.

The EARL of ROSSE, President, in the Chair.

The Statutes respecting the election of Fellows having been read, and J. G. Appold, Esq., and John Hogg, Esq. having, with the consent of the Society, been appointed Scrutators to assist the Secretaries in examining the lists, the votes of the Fellows present were collected and the following gentlemen declared duly elected:—

George James Allman, M.D.
Edward William Brayley, Esq.
Alexander Bryson, M.D.
J. Lockhart Clarke, Esq.
Joseph Dickinson, M.D.
Ronald Campbell Gunn, Esq.
Robert Hunt, Esq.
John Bennet Lawes, Esq.
Robert Mallet, Esq.
Charles May, Esq.
Capt. Thomas E. L. Moore, R.N.
Captain Richard Strachey.
Robert Dundas Thomson, M.D.
Samuel Charles Whitbread, Esq.
William Crawford Williamson, Esq.

On the motion of Sir Robert Inglis, Bart., seconded by Colonel Sabine, the thanks of the Society were given to the Earl of Rosse, and the meeting adjourned.

June 15, 1854.

The EARL of ROSSE, President, in the Chair.

The following gentlemen having paid their Fees, were admitted into the Society:—

James Apjohn, M.D.
Edward William Brayley, Esq.
Alexander Bryson, M.D.
John Bennet Lawes, Esq.
Charles May, Esq.
Captain Richard Strachey.
Robert Dundas Thomson, M.D.
Samuel Charles Whitbread, Esq.

The following gentlemen were elected Foreign Members of the Society:—

Karl Ernst von Baer.
Michel Chasles.
Friedrich Wöhler.

The following papers were read:—

I. The Bakerian Lecture.—"On Osmotic Force." By Professor Graham, V.P.R.S.

This name was applied to the power by which liquids are impelled through moist membrane and other porous septa in experiments of endosmose and exosmose. It was shown that with a solution of salt on one side of the porous septum and pure water on the other side (the condition of the osmometer of Dutrochet when filled with a saline solution and immersed in water), the passage of the salt outward is entirely by diffusion, and that a thin membrane does not sensibly impede that molecular process. The movement is confined to the liquid salt particles, and does not influence the water holding them in solution, which is entirely passive: it requires no further explanation. The flow of water inwards, on the other hand,

affects sensible masses of fluid, and is the only one of the movements which can be correctly described as a current. It is osmose, and the work of the osmotic force to be discussed.

As diffusion is always a double movement—while salt diffuses out, a certain quantity of water necessarily diffusing in at the same time in exchange—diffusibility might be imagined to be the osmotic force. But the water introduced into the osmometer in this way has always a definite relation to the quantity of salt which escapes, and can scarcely rise in any case above four or six times the weight of salt, while the water entering the osmometer often exceeds the salt leaving it, at least one hundred times. Diffusion is therefore quite insufficient to account for the water current.

The theory which refers osmose to capillarity appears to have no better foundation. The great inequality of ascension assumed among aqueous fluids is found not to exist, when their capillarity is correctly observed: and many of the saline solutions which give rise to the greatest osmose are undistinguishable in ascension from pure water itself.

Two series of experiments on osmose were described, the first series made with the use of porous mineral septa, and the second series with animal membrane. The earthenware osmometer consisted of the porous cylinder employed in voltaic batteries, about 5 inches in depth, surmounted by an open glass tube 0·6 inch in diameter, attached to the mouth of the cylinder by means of a cap of gutta percha. In conducting an experiment the cylinder was filled with any saline solution to the base of the glass tube, and immediately placed in a large jar of distilled water; and as the fluid within the instrument rose in the tube, during the experiment, water was added to the jar so as to prevent inequality of hydrostatic pressure. The rise (or fall) of liquid in the tube was highly uniform, as observed from hour to hour, and the experiment was generally terminated in five hours. From experiments made on solutions of every variety of soluble substance, it appeared that the rise or osmose is quite insignificant with neutral organic substances in general, such as sugar, alcohol, urea, tannin, &c.; so also with neutral salts of the earths and ordinary metals, and with chlorides of sodium and potassium, nitrates of potash and soda and chloride of mercury. A more sensible but still very moderate osmose is exhibited by hydrochloric,

nitric, acetic, sulphurous, citric and tartaric acids. These are surpassed by the stronger mineral acids, such as sulphuric and phosphoric acid and sulphate of potash, which are again exceeded by salts of potash and soda possessing either a decided acid or alkaline reaction, such as binoxalate of potash, phosphate of soda and carbonates of potash and soda. The highly osmotic substances were also found to act with most advantage in small proportions, producing in general the largest osmose in the proportion of one-quarter per cent. of salt dissolved. Osmose is eminently the phenomenon of weak solutions. The same substances are likewise always chemically active bodies, and possess affinities which enable them to act upon the material of the earthenware septum. Lime and alumina were accordingly always found in solution after osmose, and the corrosion of the septum appeared to be a necessary condition of the flow. Septa of other materials, such as pure carbonate of lime, gypsum, compressed charcoal and tanned sole-leather, although not deficient in porosity, gave no osmose, apparently because they are not acted upon chemically by the saline solutions. Capillarity alone was manifestly insufficient to produce the liquid movement, while the *vis motrix* appeared to be chemical action.

The electrical endosmose of Porrett, which has lately been defined with great clearness by Wiedemann, was believed to indicate the possession of a peculiar chemical constitution by water, while liquid, or at least the capacity to assume that constitution when polarized and acting chemically upon other substances. A large but variable number of atoms of water are associated together to form a liquid molecule of water, of which an individual atom of oxygen stands apart forming a negative or chlorous radical, while the whole remaining atoms together are constituted into a positive or basylous radical, which last will contain an unbalanced equivalent of hydrogen giving the molecule basicity, as in the great proportion of organic radicals. Now it is this voluminous basylous radical that travels in the electrical decomposition of pure water, and resolves itself into hydrogen gas and water at the negative pole, causing the accumulation of water observed there, while the oxygen alone proceeds in the opposite direction to the positive pole. Attention was also called to the fact that acids, and alkalies, when in solution, are chemically combined with much water of hydration, sulphuric acid for instance evolving heat

when the fiftieth equivalent of water is added to it. In the combination of such bodies, the disposal of the water is generally overlooked. Osmose was considered as depending upon such secondary results of combination, that is, upon the large number or voluminous proportions of the water molecules involved in such combinations. The porous septum is the means of bringing out and rendering visible, both in electrical and ordinary osmose, this liquid movement attending chemical combinations and decompositions.

Although the nature and *modus operandi* of chemical action producing osmose remains still very obscure, considerable light is thrown upon it in the application of septa of animal membrane. Ox-bladder was found to acquire greatly increased activity, and also to act with much greater regularity when first divested of its outer muscular coat. Cotton calico also impregnated with liquid albumen and afterwards exposed to heat so as to coagulate that substance, was sufficiently impervious, and formed an excellent septum, resembling membrane in every respect. The osmometer was of the usual bulb-form, but the membrane was supported by a plate of perforated zinc, and the instrument provided with a tube of considerable diameter. The diameter of the tube being one-tenth of that of the mouth of the bulb or the disc of membrane exposed to the fluids, a rise of liquid in the tube, amounting to 100 millimeters, indicated that as much water had permeated the membrane and entered the osmometer, as would cover the whole surface of the membrane to a depth of one millimeter, or one twenty-fifth part of an inch. Such millimeter divisions of the tube become degrees of osmose, which are of the same value in all instruments.

Osmose in membrane presented many points of similarity to that in earthenware. The membrane is constantly undergoing decomposition and its osmotic action is exhaustible. Salts and other substances, also capable of determining a large osmose, are all chemically active substances, while the great mass of neutral organic substances and perfectly neutral monobasic salts of the metals, such as chloride of sodium, possess only a low degree of action or are wholly inert. The active substances are also relatively most efficient in small proportions. When a solution of the proper kind is used, the osmose or passage of fluid proceeds with a velocity wholly unprecedented in such experiments. The rise of liquid in the tube with a

solution containing one-tenth of a per cent. carbonate of potash in the osmometer, was 167 degrees, and with 1 per cent. of the same salt 206 degrees, in five hours. With another membrane and stronger solution, the rise was 863 millimeters, or upwards of 38 inches, in the same time, and as much water was therefore impelled through the membrane as would cover its whole surface to a depth of 8·6 millimeters or one-third of an inch. The chemical action must be different on the substance of the membrane, at its inner and outer surfaces, to induce osmose; and according to the hypothetic view which accords best with the phenomena, the action on the two sides is not unequal in degree only, but also different in kind. It appears as an alkaline action on the albuminous substance of the membrane, at the inner surface, and as an acid action on the albumen at the outer surface. The most general empirical conclusion that can be drawn is, that the water always accumulates on the alkaline or basic side of the membrane. Hence, with an alkaline salt, such as carbonate or phosphate of soda in the osmometer, and water outside, the flow is inwards. With an acid in the osmometer, on the contrary, the flow is outwards, or there is negative osmose, the liquid then falling in the tube. In the last case the water outside is basic when compared with the acid within, and the flow is therefore still towards the base. The chloride of sodium, chloride of barium, chloride of magnesium, and similar neutral salts, are wholly indifferent, or appear only to act in a subordinate manner to some other active acid or basic substance, which last may be present in the solution or membrane only in the most minute quantity. Salts which admit of dividing into a basic subsalt and free acid exhibit an osmotic activity of the highest order. Such are the acetate and various other salts of alumina, iron and chromium, the protochloride of copper and tin, chloride of copper, nitrate of lead, &c. The acid travels outwards by diffusion, superinducing a basic condition of the inner surface of the membrane and an acid condition of the outer surface, the favourable condition for a high positive osmose. The bibasic salts of potash and soda, again, although strictly neutral in properties, such as the sulphate and tartrate of potash, begin to exhibit a positive osmose, in consequence, it may be presumed, of their possible resolution into an acid supersalt and free alkaline base.

The following Table exhibits the osmose of substances of all classes :—

Osmose of 1 *per cent. solutions in Membrane.*

	Degrees.		Degrees.
Oxalic acid	−148	Chloride of zinc	54
Hydrochloric acid (0·1 per c.)	− 92	Chloride of nickel	88
Terchloride of gold	− 54	Nitrate of lead	125 to 211
Bichloride of tin	− 46	Nitrate of cadmium	137
Bichloride of platinum...	− 30	Nitrate of uranium.........	234 to 458
Chloride of magnesium...	− 3	Nitrate of copper............	204
Chloride of sodium	+ 2	Chloride of copper	351
Chloride of potassium ...	18	Protochloride of tin	289
Nitrate of soda	2	Protochloride of iron	435
Nitrate of silver	34	Chloride of mercury	121
Sulphate of potash	21 to 60	Protonitrate of mercury...	356
Sulphate of magnesia ...	14	Pernitrate of mercury......	476
Chloride of calcium	20	Acetate of sesquioxide of iron	194
Chloride of barium	21	Acetate of alumina.........	280 to 393
Chloride of strontium ...	26	Chloride of aluminium ...	540
Chloride of cobalt	26	Phosphate of soda	311
Chloride of manganese...	34	Carbonate of potash	439

The osmotic action of carbonate of potash and other alkaline salts is interfered with in an extraordinary manner by the presence of chloride of sodium, being reduced almost to nothing by an equal proportion of that salt. The moderate positive osmose of sulphate of potash is converted into a very sensible negative osmose by the presence of the merest trace of a strong acid, while the positive osmose of the first-mentioned salt is singularly promoted by a small proportion of an alkaline carbonate. The last statement is illustrated by the following observations :—

Osmose in same membrane.

		Degrees.
1 per cent. sulphate of potash	....	21
Same	+0·1 per c. carb. potash ..	254
Same	+ Same	264
0·1 per cent. carbonate of potash alone	....	92
Same	....	95

It may appear to some that the chemical character which has been assigned to osmose takes away from the physiological interest of the subject, in so far as the decomposition of the membrane may appear to them to be incompatible with vital conditions, and that osmotic movement must therefore be confined to dead matter. But such apprehensions are, it is believed, groundless, or at all events premature. All parts of living structures are allowed to be in a state of incessant change, of decomposition and renewal. The decomposition occurring in a living membrane, while effecting osmotic propulsion, may possibly therefore be of a reparable kind. In other respects chemical osmose appears to be an agency particularly adapted to take part in the animal œconomy. It is seen that osmose is peculiarly excited by dilute saline solutions, such as the animal juices really are, and that the alkaline or acid property which these juices always possess is another most favourable condition for their action on membrane. The natural excitation of osmose in the substance of the membranes or cell-walls dividing such solutions seems therefore almost inevitable.

In osmose there is further a remarkably direct substitution of one of the great forces of nature by its equivalent in another force—the conversion, as it may be said, of chemical affinity into mechanical power. Now what is more wanted in the theory of animal functions than a mechanism for obtaining motive power from chemical decomposition as it occurs in the tissues? In minute microscopic cells, the osmotic movements should attain the highest velocity, being entirely dependent upon extent of surface. May it not be hoped, therefore, to find in the osmotic injection of fluids the deficient link, which certainly intervenes between muscular movement and chemical decomposition?

II. "Examination of the Cerebro-spinal Fluid." By William Turner, Esq., Scholar of St. Bartholomew's Hospital. Communicated by James Paget, F.R.S. Received May 18, 1854.

In the Bulletin de l'Académie de Médecine for December 1852, a paper is published by M. Bussy, containing an analysis by M. Des-

champs of a fluid, which flowed from the ear of a man who had sustained a fracture of the base of the cranium. From a comparison between the composition of this fluid and that given by M. Lassaigne as the composition of the cerebro-spinal fluid, M. Bussy arrived at the conclusion that they were identical in their origin. In addition, however, to the albumen and ordinary saline constituents, M. Deschamps found that the fluid obtained from the fractured cranium contained a certain constituent which possessed the peculiar property of reducing the blue protoxide of copper to the state of the yellow suboxide.

As this power of reducing the oxide of copper is possessed also by grape-sugar, M. Bussy arrived at the conclusion that this fluid contained a small portion of grape-sugar, and as additional evidence in support of this conclusion he quotes the experiments of M. Bernard, who, by irritating the base of the encephalon and the origin of the vagus nerve, produced an excess of sugar in the secretions. He supposes that in the present instance the fracture through the base of the cranium may have produced some irritation at the origin of the pneumogastric, and thus have excited the formation of sugar. Such a supposition would have received additional confirmation if at the same time an analysis could have been made of the blood, urine, or other secretions, so as to determine if sugar was present in those fluids—no such analysis however is given. The property of reducing the oxide of copper was also found by M. Bussy to reside in the cerebro-spinal fluid of the Horse and Dog. In none of these experiments was he able to induce fermentation. As this reducing power is not peculiar to grape-sugar, but is possessed by other organic substances, such as lactine and lactucine, this test alone should not be relied on as affording any positive indications of its presence; recourse should therefore be had to other confirmatory experiments.

With a view to determine this point, Mr. Paget, in the early part of March last, gave me for examination three separate portions of the cerebro-spinal fluid, obtained by puncturing a spina bifida in a child, several days intervening between the removal of each portion.

Those removed on the first two occasions were perfectly clear and pellucid, giving an alkaline reaction to test-paper, their spec. grav.

being 1·006, no spontaneous coagulation taking place after standing for some time; that removed on the third occasion had a slightly yellow tinge, and a distinct coagulum formed in it on standing. The presence of fibrine in this instance was owing doubtless to some slight inflammation having been set up, caused by the successive puncturings. The three specimens corresponded in the following characters:—

1st. No precipitate on applying heat, merely an opalescence being produced; on the addition of a few drops of nitric acid a white flaky precipitate subsided. Nitric acid alone, without heat, also caused a precipitate.

The non-precipitation of the albumen, until the addition of the acid, was owing to the alkalinity of the fluid.

2nd. Boiled with liquor potassæ a very faint pinkish tint was produced; a few white flakes also fell down.

3rd. Heated in a water-bath with the blue oxide of copper, in a few minutes the yellowish red powdery suboxide precipitated.

This reaction took place both in the original albuminous liquid and after the coagulation of the albumen by heat and nitric acid.

4th. A piece of flannel, saturated with the chloride of tin, was well moistened with the fluid, and then heated over a red-hot coal; no brown colour of the flannel was produced, such as occurs when grape-sugar is present. (Maumenè's test.)

5th. A portion mixed in a test-tube with some German yeast was placed for several hours in a warm cupboard, but there was no development of gas.

From these experiments it appears that of the various tests employed, only one gave any indication of the presence of grape-sugar, that test also being the one which is most liable to deception. The lowness of the specific gravity, in which respect this fluid and that analysed by M. Deschamps closely corresponded, would, *à priori*, almost lead to the assumption that no grape-sugar was present.

The presence of the reducing agent could not in this case depend upon any irritation of the origin of the vagus, for the irritation, if any, produced by a spina bifida is at the end of the cerebro-spinal axis furthest removed from the origin of that nerve. That the material however which effects this reduction is of a very changeable nature, was shown by allowing a portion of the fluid to stand for

several days until putrefaction had commenced. The fluid was then filtered so as to separate the insoluble albuminous flakes, and the clear liquid heated in a water-bath with the blue oxide of copper; when, instead of the suboxide being produced, the black anhydrous oxide was formed, just as is the case when the blue oxide is heated merely with water, thus satisfactorily showing that the reducing substance had been destroyed.

The recent investigations of Virchow* and Busk† have shown that substances of a non-nitrogenous nature exist both in the brain and spinal cord, but they hold somewhat different opinions respecting their exact characters; for whilst the former considers them to be cellulose, the latter regards them both in their "structural, chemical and optical properties" to resemble starch. In conformity with these views, it was interesting to determine if any indications of the presence of either of these substances could be found in the cerebro-spinal fluid; accordingly a portion of the fluid was evaporated nearly to dryness and then divided into two portions; to one was added an alcoholic solution of iodine and concentrated sulphuric acid, when a violet tint was produced, which after a few minutes disappeared; but it was also found that this same appearance was produced when the acid and iodine solution were mixed together alone, the violet colour being evidently owing to the volatilization of a part of the iodine and the evolution of its characteristic violet tint; to the other a solution of iodide of potassium and then nitric acid was added, when a brown colour was produced, owing to the liberation of the iodine. In neither portion could it be said that any evidence of the presence of starch or cellulose was detected.

A comparative trial was also made between the effects produced upon the blue oxide of copper by the cerebro-spinal fluid, solutions of grape-sugar, cane-sugar, starch, cellulose, and mannite, an unfermentizable sugar. These various substances were heated in a water-bath for the same length of time, when it was found that whilst the grape-sugar effected a reduction immediately, and the cerebro-spinal fluid only after the lapse of several minutes, neither the starch, cellulose, cane-sugar nor mannite effected any reduction at all.

The power of reducing the blue oxide of copper is not confined to

* Quarterly Journal of Microscopical Science, January 1854. † Ibid.

non-nitrogenous substances, for I found that if a solution of leucine* be heated along with it in the usual manner, the reduction is effected in about the same length of time, and in the same way as by the cerebro-spinal fluid. This single experiment is not of itself sufficient evidence that the reducing power in both cases depends upon the presence of the same substance. Such an assertion could only of course be proved by obtaining from the cerebro-spinal fluid leucine in the crystallized form. A proper quantity of the fluid was not, however, left to investigate this point.

From the above experiments I think it may be safely asserted that the power possessed by the cerebro-spinal fluid of reducing the oxide of copper, is not owing to the presence either of grape-sugar or any of the allied substances: whether it may depend upon the presence of leucine or other modifications of albumen of a somewhat similar nature, or whether it may be due to the presence of a substance belonging to another series, is a point that has yet to be determined.

Note by Mr. Paget.—The patient from whom the fluid analysed by Mr. Turner was obtained, was a girl born of healthy parents. An infant cousin had lately died from the same congenital defect as she presented. The upper part of the body was well formed, but the pelvis and lower limbs were small and nearly powerless. The sac containing the fluid was seated over the last lumbar vertebra, projecting (as the examination after death showed) through an opening between its unclosed arches. It enlarged quickly after birth, but did not evidently affect the child's health, unless it were connected with a very frequent spasmodic action of the muscles closing the glottis, which, almost from the time of birth, had produced the peculiar "crowing inspiration," or laryngismus stridulus. The fluid was first withdrawn when the child was three months old. Neither on this, nor on any subsequent occasion, did its removal produce any manifest effect, although the flaccidity of the emptied sac indicated that the pressure upon the spinal cord was greatly diminished. After every

* Leucine $C_{12}NH_{13}O_4$, a weak base, belonging to the same series as glycocine and alanine, is generally obtained by the decomposition of albuminous substances. It has been obtained by Scherer from the spleen, and, according to Gregory, has been detected as a natural product in the liver of the Calf.

evacuation the sac very quickly filled again, notwithstanding pressure exercised upon it.

The examination after death showed that the fluid was collected in the expanded tissue of the pia mater, or subarachnoid spaces, about the cauda equina. The pia mater presented appearances of inflammation long past, as well as of that which had probably been the cause of death. The canal in the axis of the spinal cord was distinct in its whole length. Commencing, below a large fourth ventricle, with a diameter of about one-fourth of a line, it gradually widened, till, at the lumbar part of the cord, it had a diameter of a line and a half. Its termination at the end of the cord could not be traced in the confusion of parts caused by the distension and inflammation of the membranes.

III. "On the Oxidation of Ammonia in the Human Body." By H. Bence Jones, M.D., F.R.S., Physician to St. George's Hospital. Received June 14, 1854.

In the last edition of Professor Lehmann's Animal Chemistry, vol. ii. p. 363, a very decided opinion is expressed against the conclusion to which I arrived in consequence of some experiments published in the Philosophical Transactions for 1851.

I considered it proved that ammonia was partly at least converted into nitrous acid in its passage through the body. In opposition to this Professor Lehmann states,—

1st. That the method which I employed must of necessity give a reaction resembling that given by nitrous acid; his words are, "Es wäre nun leicht einzusehen dass schweflige Säure, durch welche bekanntlich Iodwasserstoff zersetzt wird, in die Vorlage übergeht und so jene vermeintliche salpetersaure Reaction bedingt."

2ndly. That when nitric acid was added to urine and it was distilled with phosphoric acid instead of sulphuric acid, no trace of blue colour with starch and iodide of potassium could be obtained. "Das nach Anwendung von Phosphorsäure erhaltene Destillat giebt aber auch jene vermeintliche salpetersaure Reaction nicht, ja selbst dann nicht, wenn dem Harn vorher absichtlich einige Tropfen Salpetersäure zugesetzt worden waren."

It appeared to me undesirable merely to reply to Professor Lehmann, that I had expressly stated that the indigo and protosulphate of iron tests were used, and gave as decided proof of the presence of nitrous acid in the urine as Price's test gave; and that sulphurous acid could not have produced the same effect as nitrous acid in these tests. It seemed more desirable to repeat the experiments which had been made in Professor Lehmann's laboratory on the action of sulphurous acid, and on the effect of using phosphoric instead of sulphuric acid in the distillation of the urine.

I was fortunate enough to obtain the assistance of Mr. Malone to carry on the experiments continuously from day to day, and through the kindness of Dr. Hofmann this was done in the College of Chemistry.

1st. On the action of sulphurous acid on starch and iodide of potassium and very dilute hydrochloric acid.

In England it is by no means well known that sulphurous acid decomposes hydriodic acid. On the contrary, theoretically it should not liberate iodine, and experimentally not only does it not liberate iodine, but it hinders the liberation of iodine and stops the formation of the blue colour when Price's test is used and nitrous acid is present; and if sulphurous acid be added after the blue colour is formed it makes it disappear.

Pure sulphurous acid was prepared, some nitre was fused, and a dilute solution was made, and it was tested by Price's test (starch, iodide of potassium and very dilute hydrochloric acid), then the dilute nitre solution immediately gave the deep blue iodide of starch; but when much or little sulphurous acid was added previously to the nitre solution, no blue colour at all was produced; and when, instead of the nitre solution, much or little sulphurous acid alone was added, contrary to the statement of Lehmann, no decomposition of the hydriodic acid could be obtained.

If instead of pure iodide of potassium it was mixed with iodate of potassa, an immediate blue colour was of course observed. I can only suppose that in this way Professor Lehmann obtained the reaction which he has attributed wrongly to the action of sulphurous acid on hydriodic acid, unless indeed no sulphurous acid at all was present and the acidity of the distillate was unneutralized. Dr. Lehmann is however right as well as wrong, in saying that Price's test

for nitric acid fails when sulphurous acid is present. The test fails, not, as he says, because sulphurous acid has the same action as nitrous acid in liberating iodine, but because it has exactly the opposite property of hindering the iodide from being set free even when nitrous acid in small quantity is present.

It is possible that in distilling the urine with sulphuric acid, the distillation, if carried too far, may give rise to sulphurous acid, and that thus Price's test may fail to detect nitrous acid in the urine. Moreover, portions of the distillate may be projected against the sides of the hot retort, by which the sulphuric acid acting on the organic matter may be decomposed, and minute quantities of sulphurous acid may be liberated. This sulphurous acid, instead of decomposing hydriodic acid, causes the reformation of hydriodic acid when nitrous acid liberates iodine in Price's test.

2ndly. Lehmann states that experiments were made by distilling urine to which a few drops of nitric acid were added with phosphoric acid, and that then the distillate gave no reaction with Price's test.

The following experiments were made with every precaution.

Anhydrous phosphoric acid was prepared, and it was found to be free from nitrous acid. Some healthy urine was taken and some pure nitrate of potassa, in the proportion of two grains of salt to an ounce of fluid, and distilled with phosphoric acid (ten ounces of urine, twenty grains of nitre, and one ounce of anhydrous phosphoric acid). On concentrating, the neutralized dilute nitrous acid was detected by all the tests, namely, the indigo test, the protosulphate of iron and Price's test.

In a second experiment, five ounces of urine with five grains of nitre and half an ounce of anhydrous phosphoric acid, gave nitrous acid by all the tests. The distillation was continued until the contents of the retort were viscid.

In a third experiment, three ounces of urine with a grain and a half of nitre were distilled with three drachms of glacial phosphoric acid; the distillate neutralized and evaporated gave no trace of nitrous acid; the same urine with the same quantity of nitre and three drachms of sulphuric acid, when distilled, gave a distillate, which when neutralized and evaporated gave decided evidence of nitrous acid.

In my former paper I showed that by distilling with sulphuric

acid when only one-tenth of a grain of nitre was added to each ounce of urine, nitrous acid could be detected.

From these experiments it appears that distillation with sulphuric acid is to be preferred to distillation with phosphoric acid; but even with this last acid, when a grain of nitre is added to an ounce of urine, the nitrous acid can be detected.

I then endeavoured, by using phosphoric instead of sulphuric acid in distilling urine passed after a salt of ammonia had been taken into the stomach, to detect nitrous acid in the urine.

Two drachms of muriate of ammonia were taken in seven ounces of distilled water. The urine was collected for six hours afterwards. Twelve ounces of this urine were distilled with one ounce of phosphoric acid (anhydrous). The distillate, when concentrated, did not give any evidence of nitrous acid by Price's test.

The same experiment was repeated with no better result.

In another experiment, sulphuric acid, six drachms to twelve ounces of urine, was used instead of phosphoric acid. The distillate as soon as it was obtained gave the slightest precipitate with chloride of barium insoluble in nitric acid, showing that a trace of sulphuric acid was carried over into the receiver. The distillate was made alkaline with pure carbonate of soda, evaporated, and nitrous acid was immediately detected by the indigo and iron test, as well as by Price's test. A portion of the distillate left exposed to the air, on the following day had lost the power of liberating iodine. This arose from the nitrous acid passing into nitric acid.

Pure nitre gives no colour with starch, iodide of potassium and dilute hydrochloric acid, but when fused it produces the liberation of iodine immediately. If the solution of fused nitre is exposed to the air it loses this property, but regains it when the solution is evaporated to dryness and refused and again dissolved.

In another experiment six ounces of urine passed before the muriate of ammonia was taken were distilled with half an ounce of sulphuric acid, the distillate was highly acid, and gave a slight precipitate with chloride of barium; it was made slightly alkaline, evaporated to a small residue, and then gave no evidence of nitrous acid. Then two drachms of muriate of ammonia were taken in seven ounces of distilled water, eight ounces of urine passed four hours afterwards were distilled with half an ounce of sulphuric acid. The

distillate was fractional; the first portion gave no colour with starch test; it contained a minute trace of sulphurous acid. The second portion was highly acid; it was made slightly alkaline, evaporated nearly to dryness, and then gave most positive evidence of nitrous acid by Price's test, and also by decolorizing a deep solution of indigo.

Thus before the salt of ammonia was taken no nitrous acid could be detected in the urine, whilst after the ammonia nitrous acid was proved to be present, not only by Price's test, but by the indigo test also.

In conclusion, it results from these experiments,—1st, That in Price's test sulphurous acid produces exactly the opposite effect to nitrous acid, and even hinders nitrous acid from liberating iodine from hydriodic acid.

2ndly. That phosphoric acid, when mixed with urine containing nitre and distilled very low, does liberate nitrous acid; though when used instead of sulphuric acid, it does not enable the nitrous acid to be detected so readily as when the latter acid is employed.

Hence the experiments performed in Professor Lehmann's laboratory by Herr Jaffé*, do not invalidate Price's test for nitrous acid in the way Professor Lehmann supposes; and by again repeating some of my former experiments, I still arrive at the conclusion that when ammonia is taken into the body nitric acid may be detected in the urine, but that the quantity which can be made to appear is so small that the most delicate method is required for its detection. This however is no proof that a much larger quantity may not be lost in the process for obtaining it from the urine.

* Erdmann's Journal, vol. lix. p. 238, 1853.

IV. "On the Disintegration of Urinary Calculi by the Lateral Disruptive Force of the Electrical Discharge." By George Robinson, M.D., Licentiate of the Royal College of Physicians of London, and Lecturer on Medicine in the Newcastle-upon-Tyne College of Practical Science. Communicated by Dr. Sharpey, Sec. R.S. Received June 13th, 1854.

The great and diversified powers of electricity have long suggested the possibility of its being employed as a means of effecting the destruction of calculi in the human bladder, and thus obviating the necessity for the painful and dangerous operation of lithotomy. But the attempts hitherto made in this direction have contemplated the solution of the stone through electrolytic action rather than its disintegration by the mechanical force of the electrical discharge. A moment's reflection will however suffice to convince us that the force which shatters a steeple or cleaves an oak, is also capable of reducing to fragments the largest urinary concretion. Nor can I imagine any other than the following sources of objection to the practicability of employing this force for the purpose of breaking down vesical calculi *in situ*, namely, 1. the danger to the living structures from the necessity of using a powerful discharge; 2. the difficulty of conveying the force to the required spot, or in other words, causing the discharge to pass through the calculus. The first objection is in a great measure met by the fact of our being enabled to regulate with the utmost precision the degree of intensity of the discharge, and it would be almost entirely removed were it possible to apply the disruptive force of electricity without any portion of the body being included within the circuit traversed by the electrical current. The second objection rests upon the mechanical difficulty of bringing the calculus within the direct route of the electrical discharge, but would scarcely apply were it demonstrated that the disruptive effects of electricity can be obtained without any such direct transmission of the current.

My own attention was some years since directed to the subject by reading an account of the following experiment first performed by Mr. Crosse. "Two platinum wires one-thirtieth of an inch in

diameter were secured to a slip of window glass half an inch wide and four inches long, so that they rested upon the flat surface of the glass, leaving an interval between their points of one-twentieth of an inch. The wires were connected, one with the negative conductor of a powerful machine, the other with a ball to receive sparks from the prime conductor. On placing the glass in a flat dish filled with water and turning the machine, the glass between the points soon became fractured, and after 100 revolutions the fracture enlarged and two small cracks appeared. After 200 revolutions an excavation was formed, but on the side *opposite* to that on which the wires were tied. After 250 revolutions the glass was completely perforated. Many variations of this experiment were made, in all of which the same kind of mechanical effect was obtained. Even quartz was excavated*."

It being thus shown that a lateral disruptive action takes place within a certain distance of the seat of discharge, the idea at once suggested itself to me, that by using two parallel wires separated at their extremities like those in Mr. Crosse's experiment, and similarly connected with an electrical machine or Leyden jar, bringing their ends in contact with the surface of a calculus, and then allowing a series of moderate discharges to take place between the extremities of the wires, a disintegrating effect would be produced upon urinary calculi of the same nature as that witnessed in glass and quartz. And short of the actual disintegration of a calculus in the bladder of a living person, the following experiments will, I trust, be deemed conclusive on this point.

Two copper wires, one-twentieth of an inch in diameter, were connected, one with the external, the other with the internal surface of a Leyden jar, having about 400 square inches of internal metallic coating. These copper wires were soldered to platinum wires half an inch long and one-thirtieth of an inch in diameter. Each wire was drawn through a fine gutta percha tube, and the tubes, having first been placed perfectly parallel, were warmed and gently pressed together so as to assume somewhat of the appearance of a flexible bougie; the platinum wires projecting beyond the gutta percha to

* Described by Mr. Walker in Lardner's Cabinet Cyclopædia, vol. ii. pages 218–220.

the extent of one-eighth of an inch, and their free extremities being slightly everted and separated from each other by an interval of one tenth of an inch. In experimenting, the united gutta percha tubes were grasped and the projecting platinum points pressed against the surface of the calculus: the jar was then discharged by another person, and a series of such discharges thus passed between the free extremities of the parallel platinum wires while resting upon the surface of the stone.

With this simple arrangement, fragments a quarter of an inch long were broken off flints immersed in water, and the same force was applied to urinary calculi with the following results:—

Exp. 1. June 7th.—A piece of a large lithic acid calculus was placed in a bladder, nearly filled with water, into which the gutta percha bougie containing the wires was then introduced and the neck of the bladder tied round the instrument. The bladder with its contents being placed on a wet board, the projecting platinum wires were then kept in contact with the surface of the calculus and the jar discharged. On opening the bladder and examining the stone, it was found to be broken into numerous fragments by the single discharge.

Exp. 2.—A small phosphatic calculus, very smooth and hard, was experimented upon in a similar manner. The first five discharges produced no perceptible effect, but the sixth split it into at least twenty fragments, and many of these, on being slightly pressed between the finger and thumb, readily broke down.

Exp. 3.—A very large oxalate of lime or mulberry calculus with projecting tubercles was similarly tested, and the first discharge produced a small cavity in the surface to which the wires were applied, separating a considerable quantity of fine sand; but subsequent discharges did not act so efficiently on this very large stone.

Exp. 4.—On the following day, June 8th, the experiment was repeated in the presence of Messrs. Potter, Rayne and Furness, surgeons in Newcastle, and a small calculus, removed a few months since by the gentleman last mentioned from a young boy, was, after a few trials, split through the centre, one-half being reduced to fragments, and the other exhibiting in its interior a dark-coloured nucleus of lithic acid.

These experiments appear to demonstrate the practicability of

applying the lateral disruptive force of the electrical discharge to the disintegration of calculi in the bladder. There can be no difficulty in bringing the end of a gutta percha catheter, conveying two copper wires, in contact with the surface of a stone in the bladder, and a very simple mechanical contrivance will enable the extremities of the platinum wires to be protruded when the end of the catheter touches the calculus. By employing two wires, one connected with the positive, the other with the negative, portion of the jar or machine, not only is the intensity of the discharge increased, but the body is also prevented from forming any part of the circuit, and the risk of injury thereby materially diminished. The bladder used in the above-mentioned experiments was not at all injured, and on retaining a portion of it between the platinum wires so that the discharge passed through it, no perforation or other destructive effect took place. The gutta percha tubes, having the projecting platinum wires, were placed in the mouth without being in contact with the lips, and a discharge sent through the wires, but there was no perceptible shock. When, however, the bladder containing the stone rested upon the hand, during the act of disintegration a smart impulse was felt.

On the whole, I am of opinion that the electrical force applied in the manner indicated, will be found quite as efficient for the disintegration of calculi in the bladder as the more formidable analogous operation of lithotrity, occasionally practised. And, as regards simplicity and security, the electrical apparatus certainly appears preferable to the instruments used for crushing the stone by ordinary mechanical force.

Communications also were read from Mr. Forrester, Mr. Huxley, Mr. Joule and Prof. Thomson, Dr. Hassall, Dr. Hooker, Dr. Marcet, Mr. Brooke, Dr. Scoresby, Sir J. C. Ross, Mr. Wheatstone, Dr. Williamson and Mr. Collins*.

* Notices of these will appear in succeeding Numbers.

June 15, 1854. (Continued.)

The EARL of ROSSE, President, in the Chair.

The following papers were read:—

V. "The Attraction of Ellipsoids considered generally." By MATHEW COLLINS, Esq., B.A. Communicated by S. HUNTER CHRISTIE, Esq., M.A., Sec. R.S. &c. Received April 27, 1854.

The author commences by stating, that the attraction of an ellipsoid on a point on its surface or within it, in a direction perpendicular to one of its principal planes, is proportional to the distance of the attracted point from that plane.

This general proposition, which is an extension to *ellipsoids* of those already given for *spheroids* in Airy's Tract "On the Figure of the Earth," Prop. 8 and 10, and in MacLaurin's 4th Lemma, "De causa physica Fluxus et Refluxus Maris," he demonstrates—

1. In the case when the attracted point is on the surface of the ellipsoid.

The demonstration of this is much like those given by the above-named authors for the less general case of spheroids, and its final step is effected by Cor. 1 to Prop. 87 of the first book of the Principia.

2. When the attracted point is within the ellipsoid.

The demonstration in this case is effected by showing that an ellipsoidal shell, bounded by two similar and similarly placed ellipsoidal surfaces, exerts no attraction on a point situated anywhere within it or upon its interior surface.

The foregoing proposition shows that the attraction of an ellipsoid on any point on its surface, or within it, can be got at once from the attraction of the same ellipsoid on a point placed at the extre-

mity of an axis, and the author proceeds to show how the latter attraction can be found and reduced to elliptic functions. He then gives this proposition:

Let a, b, c be the semiaxes of a homogeneous fluid ellipsoid, and A, B, C the forces acting on points at the extremities of a, b, c, caused partly by the ellipsoid's own attractions on its parts, and partly by centrifugal forces of revolution about an axis ($2c$), or by the action of an extraneous force directed towards its centre, and varying as the distance from the centre, then the ellipsoid will preserve its form if $Aa = Bb = Cc$.

The last proposition stated in the paper is thus given: let R and r be the radii of two homogeneous concentric spheres; A and a the attractions of each on a point on the surface of the other, then $\frac{A}{R^2} = \frac{a}{r^2}$, whatever be the law of attraction as a function of the distance.

The demonstration given of the first of these two theorems is very concise, and of the second is direct and elementary.

VI. "Researches on the Impregnation of the Ovum in the Amphibia; and on the Early Stages of Development of the Embryo." (Third Series.) From the MS. papers of the late George Newport, F.R.S., F.L.S. &c. Selected and arranged by George Viner Ellis, Esq., Professor of Anatomy in University College, London. Communicated by Sir John Forbes, M.D., F.R.S. Received June 6th, 1854.

In this paper the author has given the result of further inquiries into the manner by which the frog's egg is impregnated, and has supplied in addition some very interesting facts respecting the development of the embryo during the earlier stages of its growth.

In consequence of the difficulties that arose in the course of the inquiry, and of the doubts that might be suggested by others from the difficulty of manipulating with the egg of the Amphibia unless certain precautions are taken, the author first describes the apparatus

used and the mode of proceeding he has employed; and his results show that he has successfully surmounted the obstacles to microscopic investigation caused by the opacity, the great size, and the tendency to movement inherent in the egg.

The fact of the impregnation of the ovum through the entrance of the spermatozoon into the yelk by its own movement was communicated to the Royal Society in a preceding paper*, and the original experiments there referred to as serving to establish the fact, are now detailed. In addition, the circumstances affecting the passage of the sperm-body through the thick investing envelopes are considered, and thence it is concluded, that "when there is any deficiency in the usual power, arising from an unhealthy condition of the fertilising body, or an increase in the resistance of the yelk membranes, the spermatozoon is unable to pass through the membranes into the yelk and the egg remains unfertilized."

The two small rounded bodies that appear on the surface of the yelk in the interval or *chamber* between it and the investing membrane, have been traced from their origin, through their changes, till their disappearance after the equatorial division of the yelk. The investigations as to the true import of these bodies have not been further carried out, in consequence of the untimely death of the author; but his observations have induced him to put forth the following statement regarding them, viz. "that they are usually, and perhaps invariably, at that part of the yelk at which the head of the embryo is afterwards found."

By following the changes in the segmenting yelk, evidence has been obtained of the derivation of different parts of the future being from definite segments of the yelk. Thus it has been found, that the half of the yelk on one side of the second or crucial cleft begins its subdivisions sooner than the opposite, and that the trunk and tail of the embryo are derived from this first subdividing part, whilst the head is produced from the other half.

Having ascertained so much respecting the foundation of different parts of the embryo, the author next determined that the axis or spine will primarily lie in a line with the first cleft of the yelk, though it may afterwards deviate somewhat from that line during the growth of the embryo.

* Philosophical Transactions for 1853, p. 271.

Lastly, it has been sought to discover what influence the artificial application of the spermatozoon to only one side of the egg would have upon the direction of the primary cleft of the yelk. The result of this inquiry seems, very curiously, to be, that the first cleft of the yelk will lie, under the circumstances stated, in a line with the point of the egg that has been touched with the impregnating fluid.

VII. "Contributions to the Anatomy of the Brachiopoda." By THOMAS H. HUXLEY, F.R.S. Received May 18, 1854.

In the course of the dissection of certain Brachiopoda with which I have recently been engaged, I have met with so many peculiarities which are unnoticed in the extant and received accounts of their anatomy, that although the pressure of other duties prevents me from attempting to work out the subject with any degree of completeness for the present, I yet gladly avail myself of the opportunity of communicating a few of the more important results at which I have arrived, in the hope that they may find a place in the Proceedings of the Royal Society.

My investigations were principally made upon *Rhynchonella psittacea*, for specimens of which I am indebted to Prof. Edward Forbes, while Dr. Gray obligingly enabled me to compare them with *Waldheimia flavescens* and with *Lingula*.

1. *The Alimentary Canal of Terebratulidæ.*—Professor Owen, in both his earlier and his later memoirs on the anatomy of the Terebratulidæ, describes at length the manner in which the intestine, as he states, terminates on the right side between the lobes of the mantle.

On the other hand, Mr. Hancock has declared himself unable to observe at this point any such anal aperture, and concludes from his own observations that the latter is situated on the ventral surface of the animal in the middle line, just behind the insertion of the great adductor muscle. M. Gratiolet, in a late communication to the Académie des Sciences, takes the same view. To get rid of the obvious difficulty, that this spot is covered by the shell, and therefore that if the anus existed here, there would be no road of escape for the fæces, Mr. Hancock and Mr. Woodward appear to be inclined

to suppose that some cloacal aperture must exist in the neighbourhood of the pedicle.

The existence of any such aperture, however, has recently been denied with great justice by Professor Owen.

The result of my own repeated examinations of *Rhynchonella psittacea* and of *Waldheimia flavescens* is—1. that the intestine does not terminate on the right side of the mantle as Professor Owen describes it, but in the middle line, as Mr. Hancock describes it in *Waldheimia*, while in *Rhynchonella* it inclines, after curving upwards, to the *left* side; and 2. that there is no anus at all, the intestine terminating in a rounded cæcal extremity, which is straight and conical in *Waldheimia*, curved to the left side and enlarged in *Rhynchonella*.

I confess that this result, so exceptional in its character, caused me no small surprise, and I have taken very great pains to satisfy myself of the accuracy of my conclusion; but notwithstanding the strong prejudice to the contrary, to which the known relations of the anal aperture in *Lingula* gave rise, repeated observation has invariably confirmed it.

Professor Owen's statement is, that in *Rhynchonella* (*Terebratula*) *psittacea* "the intestine inclines to the right side and makes a slight bend forwards before perforating the circumscribing membrane in order to terminate between the mantle lobes on that side."—*On the Anatomy of the Brachiopoda*, p. 152.

I find, on the contrary (figs. 1 and 2), that the intestine passes at first straight downwards in the middle line, as in *Waldheimia*, but instead of terminating in a rounded tapering extremity as in that genus, it bends upwards and then curves round to the *left* side, forming a sort of free cæcum in the visceral cavity. My reasons for believing that it is a free cæcum are these:—in the first place, no anal aperture can be detected in the mantle cavity, either on the right or left sides, although the small size of the animal allows of its being readily examined uninjured, with considerable magnifying powers.

Secondly. If the shell be removed without injuring the animal and the visceral cavity be opened from behind by cutting through its walls close to the bulb of the pedicle, it is easy not only to see that the disposition of the extremity of the intestine is such as I have described it to be, but by gentle manipulation with a needle to convince

oneself that it is perfectly unattached. And in connexion with this evidence I may remark, that the tissues of the Brachiopods in general are anything but delicate; it would be quite impossible for instance to break away the end of the intestine of *Lingula* from its attachments without considerable violence.

Thirdly. If the extremity of the intestine, either in *Rhynchonella*

Fig. 1.

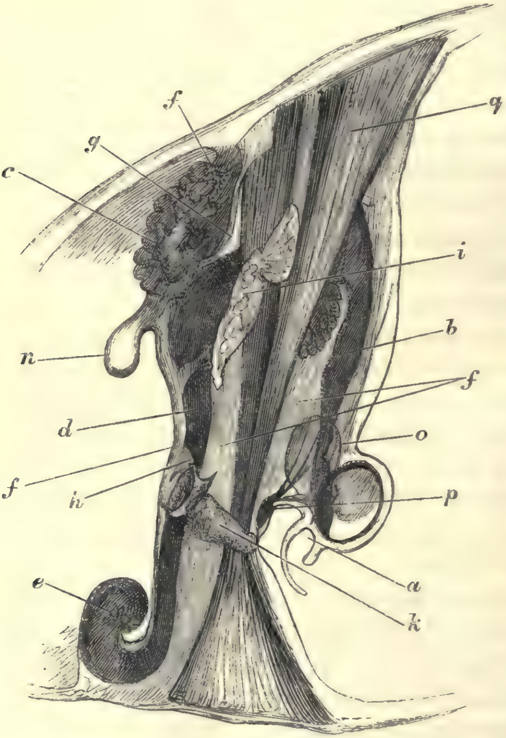


Fig. 1. *Rhynchonella psittacea*, viewed in profile; the lobes of the mantle and the pedicle being omitted.

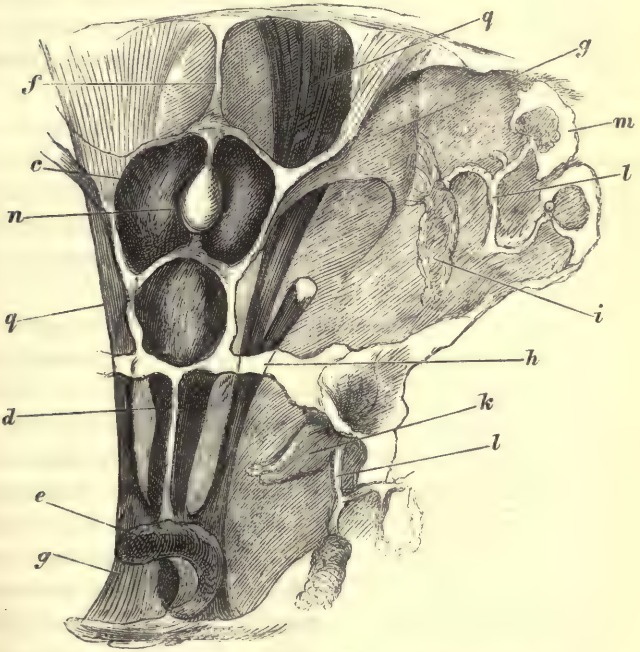
Fig. 2. The same viewed from behind, the pedicle having been cut away. The left half of the body and the liver are omitted.

a. mouth; ***b.*** œsophagus; ***c.*** stomach and liver; ***d.*** intestine; ***e.*** imperforate rectum; ***f.*** mesentery; ***g.*** gastro-parietal bands; ***h.*** ilio-parietal bands; ***i.*** superior 'heart'; ***k.*** inferior 'heart'; ***l.*** genital bands; ***m.*** openings of pallial sinuses; ***n.*** pyriform vesicle; ***o.*** sac at the base of the arm; ***p.*** ganglion; ***q.*** adductors.

or in *Waldheimia,* be cut off and transferred to a glass plate, it may readily be examined microscopically with high powers, and it is then easily observable that its fibrous investment is a completely shut sac. In *Rhynchonella* the enlarged cæcum is often full of diatomaceous shells, but it is impossible to force them out at its end, while if any aperture existed they would of course be readily so extruded.

However anomalous, physiologically, then, this cæcal termination of the intestine in a molluscous genus may be, I see no way of escaping from the conclusion that in the *Terebratulidæ* (at any rate in these two species) it really obtains. There are other peculiarities

Fig. 2.



about the arrangement of the alimentary canal, however, of which I can find either no account at all or a very imperfect notice.

The intestinal canal (figs. 1 and 2 *b, d, e*) has an inner, epithelial, and an outer fibrous coat; the latter expands in the middle line into a sort of mesentery, which extends from the anterior face of the intestine between the adductors, to the anterior wall of the visceral

chamber, and from the upper face of the intestine to the roof of the visceral chamber; while posteriorly it extends beyond the intestine as a more or less extensive free edge. I will call this the *mesentery* (*f*).

From each side of the intestinal canal, again, the fibrous coat gives off two 'bands,' an upper (*g*), which stretches from the parietes of the stomach to the upper part of the walls of the visceral chamber, forming a sort of little sheath for the base of the posterior division of the adductor muscle, which I will call the *gastro-parietal band*; and a lower, which passes from the middle of the intestine to the parietes, supporting the so-called '*auricle*.' I will call this the *ilio-parietal* band (*h*).

The ilio-parietal and gastro-parietal bands are united by certain other ridges upon the fibrous coat of the intestine, from whose point of union in the middle line of the stomach posteriorly, a pyriform vesicle (*n*) depends.

The mesentery divides the liver into two lateral lobes, while the gastro-parietal bands give rise to the appearance that these are again divided into two lobules, one above the other. I am inclined to think that these bands are what have been described as 'hepatic arteries,' at least there is nothing else that could possibly be confounded with an arterial ramification upon the liver.

This description applies more especially to *Rhynchonella* and *Waldheimia*, but the arrangement in *Lingula* is not essentially different.

2. *The Circulatory System of Terebratulidæ.*—Considerable differences of opinion have prevailed among comparative anatomists as to the nature and arrangement of the vascular system in the Brachiopoda. A pair of organs, one on each side of the body, have been recognized as Hearts since the time of Cuvier, who declared these hearts in *Lingula* to be aortic, receiving the blood from the mantle and pouring it into the body, the principal arterial trunks being distributed into that glandular mass which Cuvier called ovary, but which is now known to be the genital gland of either sex.

Professor Owen in his first memoir follows Cuvier's interpretation, stating that in *Orbicula* the pallial veins terminate in the hearts, from which arterial branches proceed to the liver and ovary. Professor Owen further adds for the Brachiopoda in general,—

"Each heart, for example, in the Brachiopoda is as simple as in

Ascidia, consisting of a single elongated cavity, and not composed of a distinct auricle and ventricle as in the ordinary Bivalves," and he compares the hearts of Brachiopoda to the auricles of Arca, &c. (Trans. Zoological Society, vol. i. p. 159).

In 1843, however, M. Vogt's elaborate memoir on *Lingula* appeared, in which the true complex structure of the 'heart' in this genus was first explained and the plaited 'auricle' discriminated from the 'ventricle;' and in 1845, Professor Owen, having apparently been thus led to re-examine the circulatory organs of Brachiopoda, published his 'Lettre sur l'appareil de la Circulation chez les Mollusques de la Classe des Brachiopodes,' in which he felicitates M. Milne-Edwards on the important confirmation of the views which the latter entertains with respect to the lacunar nature of the circulation in the Mollusca, afforded by the Brachiopoda, and describes each heart of the Terebratulidæ as consisting of a ventricle and a plaited auricle, the pallial veins not terminating in the latter but in the general visceral cavity. As the Professor does not recal the view which he had already taken of the circulation in *Orbicula*, I presume that he considers two opposite types of the circulatory organs to obtain in the Brachiopoda, the direction of the current being from the mantle through the heart towards the body in *Orbicula*, and from the mantle through the body towards the heart in *Terebratula*.

The possibilities of nature are so various that I would not venture, without having carefully dissected *Orbicula*,—no opportunity of doing which has yet presented itself,—to call this view in question, but I think it seems somewhat improbable. Indeed the structural relations which I have observed and which are described below, do not appear to me to square with any of the received doctrines of Brachiopod circulation, but I offer them simply as facts, not being prepared at present to present any safe theory on the subject.

In *Waldheimia flavescens* there are two 'hearts,' situated as Professor Owen describes them, but so far as I have been able to observe, the ventricle cannot be described as an 'oval' cavity, inasmuch as it is an elongated cavity bent sharply upon itself. Hastily examined of course this may appear oval. I have been similarly unable to discover 'the delicate membrane of the venous sinuses,' which is said by Professor Owen to "communicate with and close the basal apertures of the auricles," or to perceive that the auricular

cavity can be "correctly described as a closed one, consisting at the half next the ventricle, of a beautifully plicated muscular coat in addition to the membranous one, but at the other half next the venous sinus of venous membrane only; the latter might be termed the auricular sinus, the former the auricle proper."

I presume that 'this delicate membrane of the venous sinuses' is what I have called the ilio-parietal band, in which the base of the auricle is as it were set, like a landing-net in its hoop, but this does *not* close the base of the auricle, the latter opening widely into the visceral chamber.

I have equally failed in detecting any arteries continued from the apices of the ventricles; and I have the less hesitation in supposing I have not overlooked them, as Mr. Albany Hancock, whose works are sufficient evidence of the value of his testimony, permits me to say that he long since arrived at the conclusion that no such arteries exist.

What has given rise to the notion of the existence of these arteries appears to me to be this. A narrow band resembling those I have already described, is attached in *Waldheimia* along the base of the 'ventricle' and the contiguous outer parietes of the auricle: inferiorly it passes outwards to the sinuses, and running along their inner wall, forms a sort of ridge or axis* from which the genitalia, whether ovaria or testes, are developed, stretching through their whole length and following the ramifications of the sinuses. It is the base of these ridges seen through the walls of the sinuses, where they extend beyond the genitalia, which have been described as arteries.

The upper end of the band passes into the sinuses of the upper lobe of the mantle, and comes into the same relation with the genitalia which they enclose.

The walls of the auricle in *Waldheimia* are curiously plaited, but I have been unable, in either auricle or ventricle, to detect any such arrangement of muscular fibres as that which has been described. The epithelial investment of the auricle, on the other hand, is well developed, and in the ventricle the corresponding inner coat is raised up into rounded villous eminences.

The ventricle lies in the thickness of the parietes, while the auricle floats in the visceral cavity, supported only by the ilio-parietal band.

* This arrangement is, I find, particularly described by M. Gratiolet.

The former is at first directed downwards, but then bends sharply round and passes upwards to terminate by a truncated extremity close to the subœsophageal ganglion and bases of the arms.

Mr. Hancock informs me, that in his dissections he repeatedly found an aperture by which the apex of the 'ventricle' communicated with the pallial cavity; and that, taking this fact in combination with the absence of any arteries leading from this part, he had been tempted to doubt the cardiac nature of these organs altogether, and to regard them rather as connected with the efferent genital system, had not the difficulty of determining whether these apertures were artificial or natural prevented his coming to any definite conclusion at all.

Before becoming acquainted with Mr. Hancock's investigations, I had repeatedly observed these apertures in *Rhynchonella,* but preoccupied with the received views on the subject, I at once interpreted them as artificial. A knowledge of Mr. Hancock's views, however, led me to reconsider the question, and I have now so repeatedly observed these apertures both in *Waldheimia* and in *Rhynchonella,* that I am strongly inclined to think they may after all be natural.

If these organs be hearts, in fact, *Rhynchonella* is the most remarkable of living Mollusks, for it possesses *four* of them. Two of these occupy the same position as in *Waldheimia*, close to the origins of the calcareous crus (*k*), while the other two are placed above these, and above the mouth, one on each side of the liver (*i*). It is these latter which Professor Owen describes, while he has apparently overlooked the other two, at least he says (speaking as I presume of *Rhynchonella*) (*l. c.* p. 148) that the venous sinuses "enter the two hearts or dilated sinuses which are situated exterior to the liver, and in *T. Chilensis* and *T. Sowerbii* just within the origins of the internal calcareous loop."

The fact is, that while the ilio-parietal bands support two 'hearts' as usual, the gastro-parietal bands are in relation with two others. The base of the 'auricle' of the latter opens into the re-entering angle formed by the gastro-parietal band with the parietes, while its apex is directed backwards to join the ventricle, which passes downwards and backwards along the posterior edge of the posterior division of the adductor muscle.

The auricles in *Rhynchonella* are far smaller, both actually and

proportionally, than in *Waldheimia*. They exhibit only a few longitudinal folds, and not only present the same deficiency of muscular fibres as those of *Waldheimia*, but are so tied by the bands which support them that it is difficult to conceive how muscular fibres, even if they existed, could act. The 'ventricles' in like manner lie obliquely in the parietes of the body, and simply present villous eminences on their inner surface, which has a yellowish colour.

All these 'hearts' exhibit the same curious relation with the genitalia in *Rhynchonella* as in *Waldheimia*; that is to say, a 'genital band' (*l*) proceeds from the base of the 'ventricle' and becomes the axis of the curiously reticulated genital organ. But in *Rhynchonella* the genital bands of the upper genitalia come from their own 'hearts.'

The arrangement of the genitalia in *Rhynchonella* is very remarkable. The sinuses have the same arrangement in each lobe of the mantle. The single trunk formed by the union of the principal branches in each lobe opens into the inner and anterior angle of a large semilunar sinus which surrounds the bases of the adductors, and opens into the visceral cavity. The floor of this great sinus is marked out into meshes by the reticulated genital band, and from the centre of each mesh a flat partition passes, uniting the two walls of the sinus, and breaking it up into irregular partial channels.

There are the same anastomosing bands uniting the gastro-parietal and ilio-parietal bands on the stomach in *Rhynchonella* as in *Waldheimia*, and a pyriform vesicle of the same nature, but I did not observe in *Rhynchonella* those accessory vesicles upon the origins of genital bands, which I observed once or twice in *Waldheimia*.

I could find no trace of arteries terminating the elongated, ovoid and nearly straight 'ventricles' of *Rhynchonella*; their ends appeared truncated, and as I have already said, repeatedly presented a distinct external aperture.

Such appear to me to be the facts respecting the structure of the so-called hearts in the *Terebratulidæ*; what I believe to be an important part of their peripheral circulatory system, has not hitherto, so far as I am aware, received any notice.

In *Waldheimia* the membranous walls of the body, the parieto-intestinal bands and the mantle, present a very peculiar structure; they consist of an outer and an inner epithelial layer, of two corre-

sponding fibrous layers, and between them of a reticulated tissue, which makes up the principal thickness of the layer, and in which the nerves and great sinuses are imbedded.

The trabeculæ of this reticulated tissue contain granules and cell-like bodies, and I imagined them at first to represent a fibro-cellular network, the interspaces of which I conceived were very probably sinuses. Sheaths of this tissue were particularly conspicuous along the nerves. On examining the arms, however, I found that the oblique markings, which have given rise to the supposition that they are surrounded by muscular bands, proceeded from trabeculæ of a similar structure, which took a curved course from a canal which lies at the base of the cirri (not the great canal of the arms, of course) round the outer convexity of the arm, and terminated by breaking up into a network. These trabeculæ, however, were not solid but hollow, and the interspaces between them were solid. The network into which they broke up was formed by distinct canals, and then, after uniting with two or three straight narrow canals which ran along the outer convexity of the arm close to its junction with the interbrachial fold, appeared to become connected with a similar system of reticulated canals which occupied the thickness of that fold.

It was the examination of the interbrachial fold, in fact, which first convinced me that these reticulated trabeculæ were canals; for it is perfectly clear that vessels or channels of some kind must supply the proportionally enormous mass of the united arms with their nutritive material, and it is so easy to make thin sections of this part, that I can say quite definitely that no other system of canals than these exists in this locality.

The *facts*, then, with regard to the real or supposed circulatory organs of the *Terebratulidæ*, are simply these :—

1. There are two or four organs (hearts), composed each of a free funnel-shaped portion with plaited walls, opening widely into the visceral cavity at one end, and at the other connected by a constricted neck, with narrower, oval or bent, flattened cavities, engaged in the substance of the parietes. The existence of muscular fibres in either of these is very doubtful. It is certain that no arteries are derived from the apex of the so-called ventricle, but whether this naturally opens externally or not is a point yet to be decided.

2. There is a system of ramified peripheral vessels.

3. There are one or more pyriform vesicles.

4. There are the large 'sinuses' of the mantle, and the 'visceral cavity' into which they open.

To determine in what way these parts are connected and what functions should be ascribed to each, it appears to me that much further research is required.

Nervous System of Terebratulidæ.—Professor Owen describes and figures the central part of this system as a ring surrounding the oral aperture, its inferior portion being constituted by a mere commissural band.

M. Gratiolet, however, states with justice that the inferior side of this collar is the thicker, and I find both in *Rhynchonella* and in *Waldheimia* that it constitutes, in fact, a distinct oblong ganglion, of a brownish colour by reflected light. From its extremities commissural branches pass round the mouth, while other cords are distributed to the arms, to the superior and inferior pallial lobes, and to the so-called hearts. The nerves are marked by fine and distinct longitudinal striations, and can be traced to the margins of the pallial lobes, where they become lost among the muscular fibres of the free edges of the mantle.

Structure of the Arms.—I have not been able to convince myself of the existence of that spiral arrangement of the muscular fibres of the arms which has been described in *Rhynchonella* and *Waldheimia*. I have found the wall of the hollow cylinder of the arm to be constituted (1) externally, by an epithelium, within which lie (2) the reticulated canals, which have been already described; (3) by a delicate layer of longitudinal or more oblique and transverse fibres, which are probably muscular, and (4) internally by a granular epithelial layer.

In *Rhynchonella* the bases of the arms are terminated by two considerable sacs, which project upwards into the visceral cavity. Have these the function of distending and so straightening the spirally coiled, very flexible arms of this species?

Affinities of the Brachiopoda.—All that I have seen of the structure of these animals leads me to appreciate more and more highly the value of Mr. Hancock's suggestion, that the affinities of the Brachiopoda are with the Polyzoa. As in the Polyzoa, the flexure of

the intestine is neural, and they take a very natural position among the neural mollusks between the Polyzoa on the one hand, and the Lamellibranchs and Pteropoda on the other.

The arms of the Brachiopoda may be compared with those of the Lophophore Polyzoa, and if it turns out that the so-called hearts are not such organs, one difference will be removed.

In conclusion, I may repeat what I have elsewhere adverted to, that though the difference between the cell of a Polyzoon and the shell of a Terebratula appears wide enough, yet the resemblance between the latter with its muscles and the Avicularium of a Polyzoon, is exceedingly close and striking.

VIII. "An Inquiry into some of the circumstances and principles which regulate the production of Pictures on the Retina of the Human Eye, with their measure of endurance, their Colours and Changes."—Part II. By the Rev. WILLIAM SCORESBY, D.D., F.R.S., Corresp. Inst. of France, &c. Received May 31, 1854.

This second part of the author's inquiries concerning phenomena in optical spectra, embraced the results in respect of *colour* in the images impressed on the retina, as derived simply from the influence of *light*.

The optical spectra from white, grey, or black opake objects under faint illumination, or of ordinary windows or apertures transmitting low degrees of light, were usually found to be without colour. But ordinary daylight, and, much more, the light from bright sunshine (as is well known), yield *chromatic spectra* of vivid or brilliant hues. By viewing with slightly closed eyes, the pictures impressed on the retina by a few seconds' steady gazing at some fixed point of an illuminated object, and noting the various effects, disappearances and changes, a considerable number of characteristic phenomena were elicited, and the effects of a variety of modifying circumstances satisfactorily determined. The most prevailing influences in modifying the phenomena—whatever other causes might tend to the production of variation in the colours—were found to be referable to differ-

ences in the degree of intensity of the external light, in the extent of time occupied in gazing at the illuminated object, in the quantity of light penetrating the chamber of the eye whilst examining the spectra, and in the normal condition of the eye itself. These, with other modifying circumstances, had been somewhat elaborately investigated.

Different degrees of light, whether reflected from white objects, or transmitted by colourless glass, had obviously the tendency to yield differences in the colours of the primarily developed pictures on the retina, with corresponding varieties in the nature and number of the subsequent changes. Thus the viewing for a few seconds of an aperture in a window the size of a pane of glass, whilst all the rest was covered with a thick brown-paper screen, gave, *with a low degree of daylight*, transparent pictures of a *dingy* orange, olive, yellow-grey or bluish black tint, changing, most usually, into a rusty-tinted blackish spectrum, and disappearing, for the most part, in a minute of time or less. From *medium degrees of daylight*, the primary pictures embraced a considerable variety of colours, such as crimson-pink, purple-pink, violet, purple, indigo, blue,—the blue being the highest in the scale of intensity. The most marked changes, commencing with *blue*, were usually from blue to red, or to crimson, olive, black fading into blackish grey. In certain cases rapid and evanescent glances were had of several intermediate colours. The general photochromatic effects of the *higher degrees of light*, such as from a clear sky in full sunshine, were far more uniform than those from inferior light. The spectrum first elicited, even after viewing a window or window-aperture for three or four seconds only, was almost always *green*, with the character of illuminated transparency; the shades of colour however varied with the intensity of the impression. The picture always appeared within four or five seconds after closing the eyes, and when the light had been strong and the gazing continued for a quarter of a minute or more, the picture would burst out almost instantly. The restoration of the picture in new colours, after the vanishing, had very much the character and appearance of the dissolving views effected by the magic lanthorn. *The frame* of the window or aperture, and the crossbars, were always pictured in colours different from those of the panes, besides a fine marginal line of another colour dividing the

glass and the frames. These *consequential colours*, constitute, as is well known, a remarkable feature in the phenomena. They have generally a certain complementary relation, or tendency to such, to the colours of the primary picture. Thus in the clear green or blue spectrum of a window, derived from strong illumination, the remainder of the field of the eye will generally, in *the first* instance, be covered with a ground of glowing crimson, with cross-bars similar, and purple edgings; and when the picture changes to crimson or red, the antagonistic tint will also change, perhaps to purple, or orange or brown. The original spectra were found to fade away at intervals, often of tolerable equality, such as of eight or nine seconds, disappearing perhaps for two or three seconds, and then reappearing under, generally, some change of shade or tint, through an extent of very numerous repetitions. The changes of colour from the bright or emerald green, as very frequently traced, went rapidly through yellow-green, yellow, orange, red, scarlet, crimson and brown, or olive. And this series, it is observable, is particularly accordant, in respect to the principal or fundamental colours, with that of the prismatic spectrum from green to yellow, orange and red. These visual photographs, besides having the sharpest definition, and often the most brilliant illuminated colours, were found to possess, under strong intensities of impression, a remarkable degree of permanency—extending sometimes to endurance for an hour or longer after the act of gazing.

Investigations on the relation of the photochromatic developments to the time of gazing, gave results in many respects corresponding with those derived from differences in the degree of external light. Thus the higher colours of the spectral series elicited by strong light, could, within certain limits, be also developed by more continuous gazing with inferior light: so that the pink-coloured spectrum derived from ten to twenty seconds' gazing in low degrees of light, could be elicited by a single glance under bright sunshine. The results, therefore, were clearly in relation to the intensity of the impression; and, taken in the form of a general proposition, we shall not be far wrong, perhaps, in considering the intensity of impression as the product of the time of gazing into the relative quantity of light admitted by the aperture.

The relation of the colours primarily elicited to the intensity of

the impression, yielded (comparatively and roughly taken) the following series,—crimson-pink, purple-pink, purple, blue, green, the latter being the produce of the highest intensity tried.

As in the foregoing researches, the relative degrees of light were but broadly assumed, whilst the comparative experiments comprised a variety of differences affecting the photochromatic results, another series of experiments on the simple effects of degrees of light was instituted, in which all these other differences were eliminated. In this series the quantity of light was varied by partial or sectional screens of glass, or other transparent or semitransparent substances. The results were particularly satisfactory,—different tints or shades of colour being obtained by the same view and in the same spectrum of a window-aperture, when different thicknesses of window glass were placed in the several sections (six in number) into which the aperture was divided.

A beautiful example of the chromatic effects of partial and varied screening of light on the optical spectrum elicited, was incidentally obtained by viewing an aperture in the clouds, when the sky was otherwise densely covered. After gazing for a few seconds on the middle of this aperture, the spectrum, as viewed with gently closed eyes, exhibited a singular variety of the richest tints according to the differences in the light screened off by the edges of the cloud and by certain little patches within the aperture. The spectrum resembled the variegation and richness of colouring as elicited in certain transparent or semitransparent substances when examined by polarized light.

The experiments on binocular and multiple spectra, as described in Part I. of the author's paper, being repeated under degrees of light adequate for yielding *colour*, gave pictures, in many cases, of much interest and beauty. The multiple spectra, however, which proved the most strikingly beautiful, were derived from the sun, which was viewed *indirectly*, and on occasions, near setting, in winter, when the intensity of its light was duly subdued by passing through a dense condition of atmosphere. Under such circumstances, images, sometimes in 100 to 150 repetitions, were impressed on the retina by rapid glances at the sky immediately around the sun. These were taken by quick movements of the head, winking intermediately, at the rate of 60 to 120 impressions in the minute; and the result,

when viewed with closed eyes, presented a splendid spectacle like a cluster of coloured stars, or rather of round planetary discs, brilliant in green, yellow, orange, red, crimson and purple!

Besides the experiments thus far described, in which the spectral images were viewed, for the most part, with gently closed eyes kept steadily in the direction in which the objects were gazed on,—the differences, which were often very remarkable, produced by alterations in the quantity of light admitted into the chamber of the eye whilst the image was viewed, were also investigated. Sometimes the smallest change in the light thus transmitted was found to alter greatly the character of the spectrum. In certain cases, the compressing of the eyelids, or the mere passing of the hands betwixt the eyes and the light, would serve to change a negative picture into a positive, or the colours, as viewed in the usual way, into their complementary tints.

The paper concludes with a considerable series of deductions, applications and general results.—1. As to the *elucidation* yielded by these ocular spectra, of the theory of vision.—2. Of the principles of binocular and simple vision.—3. Of the action of the retina for the obliterating of impressed images, and the recovery of a normal condition.—4. Of the nature of certain disturbing and dazzling effects of vision by strong light.—5. Of the phenomena of certain spectral illusions.—6. As to the *practical use* of the process of examining the ocular spectra, for the determination of quantities of light relatively intercepted by different portions or thicknesses of glass or other transparent media.—7. For assisting in the determination of the relative degrees of illumination of lamps, candles, &c., and of quantities of light reflected from opake objects.—8. For aiding in the selection and harmonizing of colours in ornamental and decorative departments of art.—9. For the examination of the condition of the interior of the eyes in certain states of disease. The author having had the opportunity of trying this process in case of amaurosis, found that it afforded a perfect picture of defects in the surface of the retina of the eyes separately, when there was no visible defect, and when the patient had no other perception of a diseased eye, or patch on the retinal surface, except the partial distortion or interruption of vision. Founded on this, the author suggests a plan of *scotometrical* examination of retinal defects, by which not only the accurate

form and relative proportions of diseased patches on the retina may be determined, but their actual dimensions may probably be deduced.

IX. "On the frequent occurrence of Indigo in Human Urine, and on its Chemical, Physiological and Pathological Relations." By ARTHUR HILL HASSALL, M.D., Member of the Royal College of Physicians, Physician to the Royal Free Hospital, &c. Communicated by Professor SHARPEY, Sec. R.S. Received June 10, 1854.

The present communication embraces some further observations and experiments on the occurrence of indigo in human urine. From these it appears that the presence of that substance is even more common than the author was led to anticipate from his first inquiries, the results of which were communicated to the Society in June last.

The author furnishes additional proofs of the blue colouring matter in question being really indigo, by converting it into isatine and aniline; for this purpose it was necessary to obtain the pigment in considerable quantity.

Contrasting its chemical and physiological relations with hæmatine and urine pigment, he shows that indigo is closely allied in its nature and origin to those substances, and he considers that when indigo is met with in urine in considerable amount, it forms a vehicle for the elimination of any excess of carbon contained in the system. This view is borne out by the important fact, that the greater number of cases in which indigo has been observed to be developed in the urine in large amount have been cases of extensive tubercular disease of the lungs, and in which the decarbonizing functions of those organs are greatly impaired.

X. "On the Effect of the Pressure of the Atmosphere on the Mean Level of the Ocean." By Captain Sir JAMES CLARK ROSS, R.N., F.R.S. Received June 15, 1854.

The author states that, in September 1848, Her Majesty's ships Enterprize and Investigator having anchored in the harbour of Port Leopold in lat. 74° N. and long. 91° W., a heavy pack of ice was driven down upon and completely closed the harbour's mouth, thus effectually preventing their egress, and compelling them there to pass the winter of 1848–49. It was during that period that the series of observations here presented to the Royal Society was obtained; and, as the observations were made under peculiarly favorable circumstances, the author considers they will throw some light on the movements of the tides, and on some of the causes of their apparent irregularities.

Soon after the harbour had been completely frozen over, a very heavy pressure from the main pack forced the newly-formed sheet of ice, which covered the bay, far up towards its head, carrying the ships with it into such shallow water that at low spring-tides their keels sometimes rested on the ground. Under these circumstances the movements of the tides became to the author an object of great anxiety, and consequently of careful observation, in order to ascertain the amount of irregularities to which they were liable in that particular locality.

The first few days' observations evidenced much larger differences in the elevation or depression of successive high or low-waters than could be accounted for by any of the generally received causes of disturbance; and the author was at once led to connect them with changes of the pressure of the atmosphere, from perceiving that on the days of great atmospheric pressure high-water was not so high as it ought to have been, and low-water was lower than its proper height; and that the reverse took place on the days of smaller pressure.

As it was found that the usual method of determining the mean level of the sea, by taking the mean of successive high- and low-waters, was inadequate to the detection of small quantities arising from a change in the pressure, a system of observation was adopted

different from that heretofore practised, in order to determine the mean level of the sea on each day.

In the first instance, simultaneous observations of the height of the tide and of the mercury in the barometer were made every quarter of an hour throughout the twenty-four hours. From these it was found that the mean level of the sea for each day could be determined with great accuracy, and that the variation in the daily mean level and in the mean pressure of the atmosphere followed each other in a remarkable manner, so that a rise in the former corresponded to a diminution in the latter. Subsequently however hourly observations were adopted.

The peculiar advantages of the position of the ships at Port Leopold for making tidal observations are stated to have consisted in :—

1. The great width of the entrance of the harbour admitting the free ingress and egress of the water, combined with the large field of ice which covered the whole of the bay, completely subduing every undulation of the water.

2. The steady movement of the immense platform of ice, rising and falling with such singular regularity and precision as to admit the reading off the marks of the tide-pole with the greatest exactnesss, even to the tenth of an inch.

3. The shallowness of the water and the evenness and solidity of the clay bottom admitting the fixture of the tide-pole with immoveable firmness.

4. The whole surface of the sea in the neighbourhood being, for the greater part of the time, covered by a sheet of ice, preventing those irregularities which occur in other localities from the violence of the wind raising or depressing the sea in as many different degrees as it varied in strength or duration.

For fixing the tide-pole for the "Enterprize" a hole 2 feet square was cut through the icy platform, and a strong pole, nearly 40 feet long, was passed through it and driven firmly down several feet into the clay, being fixed by heavy iron weights, which also rested on the clay and prevented any movement of the pole. It was placed in about 21 feet depth of water at the time of mean level of the sea. Another such tide-pole was, in a like manner, fixed through a hole in the ice close to the "Investigator," for the sake of reference and comparison.

Hourly observations of the height of the tide and of the barometer were commenced on the 1st of November, and were continued by the officers of each ship throughout the whole of the nine following months to the end of July. After forty-seven days of observation an interruption in one of the series occurred in consequence of the tide-pole of the "Enterprize" having been drawn up by the ice, to the under part of which it had become frozen. The amount of displacement of the pole was easily determined by a comparison with that of the "Investigator," but several days elapsed before it could be satisfactorily fixed at the same point in which it had been originally. The observations of these forty-seven days are those which are given in the paper, and their discussion is the immediate object of the communication.

It is stated that subsequent observations seem to show that, from the time of the interruption to the middle of July, there was a progressive elevation of the mean level of the sea, which, although of small amount, was sufficiently evident from month to month to render the subdivision of the series desirable, in order that the individual observations of each separate division should be strictly comparable.

The height of the sea and the corresponding height of the mercury in the barometer, at every hour in each day, from the 1st November to the 18th December 1848 are given in tables. In these the arithmetic mean of the hourly heights of the sea for each day is taken as the mean level of the sea for that day, and the mean of the hourly heights of the barometer is taken as the corresponding height of the barometer. These mean levels and corresponding mean barometric heights are given in another two-column table, arranged in the order of the days of observation; and in a third table these are arranged in the order of the heights of the barometer with the corresponding mean levels, without regard to the dates of observation, for the purpose of showing the dependence which the latter have on the former.

On these tables the author makes the following remarks. The forty-seven days of hourly observations give for the mean height of the barometer 29·874 inches, and of the mark of the mean level of the sea 21 feet 0·21 in.

The mean of three days *greatest* pressure was	30·227, and of corresponding level 20 feet 8·4 inch.
The mean of three days *least* pressure was ...	29·559, and of corresponding level 21 feet 5·4 inch.
Diff. +0·668	Diff. −9·0

Thus a difference of pressure equal to 0·668 inch produced a difference of 9 inches in the mean level of the sea. As the ratio of 9 to ·668 is 13·467 to 1, the author considers that the effect of the pressure of the atmosphere on the level of the sea is 13·467 times as great as the effect it produces on the mercury in the barometer, or very nearly in the inverse ratio of the specific gravities of sea-water and mercury. He however states that this remarkable coincidence must be considered in a great measure accidental, for if a greater number of days' observation be taken in order to deduce the mean greatest and mean least pressure, and the corresponding mean levels, a different result will be obtained. From these observations however he considers that he has been enabled to deduce results which plainly point to the law which governs the effect of the pressure of the atmosphere on the mean level of the sea, and may be encouraged to pursue the investigation through a more extended series of observations, in order to arrive at the most accurate conclusion that the observed facts may justify.

In conclusion a formula is given for determining the correct height of the tide, or of the mean level of the sea:—

Let L denote the correct height of the tide, or of the mean level of the sea;

B the mean pressure of the atmosphere;

λ the observed height of the tide, or of the mean level of the sea;

β the corresponding height of the barometer;

D the ratio of the specific gravity of mercury to that of sea-water:

then $L=\lambda+(\beta-B)D$.

Examples are given of the application of this formula.

XI. "On the Thermal Effects of Fluids in Motion."—No. II. By J. P. Joule, Esq., F.R.S. and Professor W. Thomson, F.R.S. Received June 15, 1854.

The first experiments described in this paper show that the anomalies exhibited in the last table of experiments, in the paper preceding it*, are due to fluctuations of temperature in the issuing steam consequent on a change of the pressure with which the entering air is forced into the plug. It appears from these experiments, that when a considerable alteration is suddenly made in the pressure of the entering stream, the issuing stream experiences remarkable successions of augmentations and diminutions of temperature, which are sometimes perceptible for half an hour after the pressure of the entering stream has ceased to vary.

Several series of experiments are next described in which air is forced (by means of the large pump and other apparatus described in the first paper) through a plug of cotton wool, or unspun silk pressed together, at pressures varying in their excess above the atmospheric pressure, from five or six up to fifty or sixty pounds on the square inch. By these it appears that the cooling effect which the air, as found in the authors' previous experiments, always experiences in passing through the porous plug, varies proportionally to the excess of the pressure of the air on entering the plug above that with which it is allowed to escape. Seven series of experiments, in each of which the air entered the plug at a temperature of about 16° Cent., gave a mean cooling effect of about ·0175° Cent., per pound on the square inch, or ·27° Cent. per atmosphere, of difference of pressure. Experiments made at lower and at higher temperatures showed that the cooling effect is very sensibly less for high than for low temperatures, but have not yet led to sufficiently exact results at other temperatures than that stated (16° Cent.) to indicate the law according to which it varies with the temperature.

Experiments on carbonic acid at different temperatures are also described, which show that at about 16° Cent., this gas experiences $4\frac{1}{2}$ times as great a cooling effect as air. They agree well at all the

* Communicated to the Royal Society, June 1853, and published in the Transactions.

different temperatures with a theoretical result derived according to the general dynamical theory from an empirical formula for the pressure of carbonic acid in terms of its temperature and density, which was kindly communicated by Mr. Rankine to the authors, having been investigated by him upon no other experimental data than those of Regnault on the expansion of the gas by heat and its compressibility.

Experiments were also made on hydrogen gas, which, although not such as to lead to accurate determinations, appeared to indicate very decidedly a cooling effect amounting to a small fraction, perhaps about $\frac{1}{16}$, of that which air would experience in the same circumstances.

The following theoretical deductions from these experiments are made :—

I. The relations between the heat generated and the work spent in compressing carbonic acid, air and hydrogen, are investigated from the experimental results. In each case the relation is nearly that of equivalence, but the heat developed exceeds the equivalent of the work spent, by a very small amount for hydrogen, considerably more for air, and still more for carbonic acid. For slight compressions with the gases kept about the temperature 16°, this excess amounts to about $\frac{1}{77}$ of the whole heat emitted in the case of carbonic acid, and $\frac{1}{420}$ in the case of air.

II. It is shown by the general dynamical theory, that the air experiments, taken in connexion with Regnault's experimental results on the latent heat and pressure of saturated steam, make it certain that the density of saturated steam increases very much more with the pressure than according to Boyle's and Gay-Lussac's gaseous laws, and numbers are given expressing the theoretical densities of saturated steam at different temperatures, which it is desired should be verified by direct experiments.

III. Carnot's function in the "Theory of the Motive Power of Heat" is shown to be very nearly equal to the mechanical equivalent of the thermal unit divided by the temperature from the zero of the air-thermometer (that is, temperature Centigrade with a number equal to the reciprocal of the coefficient of expansion added), and corrections, depending on the amount of the observed cooling effects in the new air experiments, and the deviations from the gaseous laws of

expansion and compression determined by Regnault, are applied to give a more precise evaluation.

IV. An absolute scale of temperature, that is, a scale not founded on reference to any particular thermometric substance or to any special qualities of any class of bodies, is founded on the following definition :—

If a physical system be subjected to cycles of perfectly reversible operations and be not allowed to take in or to emit heat except in localities, at two fixed temperatures, these temperatures are proportional to the whole quantities of heat taken in or emitted at them respectively during a complete cycle of the operations.

The principles upon which the unit or degree of temperature is to be chosen, so as to make the difference of temperatures on the absolute scale, agree with that on any other scale for a particular range of temperatures. If the difference of temperatures between the freezing and the boiling-points of water be made 100° on the new scale, the absolute temperature of the freezing-point is shown to be about 273·7 ; and it is demonstrated that the temperatures from the freezing-point on the new scale will agree very closely with Centigrade temperature by the standard air-thermometer; quite within the limits of the most accurate practical thermometry when the temperature is between 0° and 100° Cent., and very nearly if not quite within these limits for temperatures up to 300° Cent.

V. An empirical formula for the pressure of air in terms of its density, and its temperature on the absolute scale, is investigated, by using forms such as those first proposed and used by Mr. Rankine, and determining the constants so as to fulfil the conditions (1) of giving the observed cooling effects, (2) of agreeing with Regnault's observations on expansion by heat, and (3) of agreeing with Regnault's experimental results on compressibility at a particular temperature.

A table of comparison of temperature by the air-thermometer under varied conditions of temperature and pressure with the absolute scale, is deduced from this formula.

Expressions for the specific heats of any fluid in terms of the absolute temperature, the density, and the pressure, derived from the general dynamical theory, are worked out for the case of air according to the empirical formula; and tables of numerical results derived

exclusively from these expressions and the ratio of the specific heats as determined by the theory of sound, are given. These tables show the mechanical values of the specific heats of air at different constant pressures, and at different constant densities. Taking 1390 as the mechanical equivalent of the thermal unit as determined by Mr. Joule's experiment on the friction of fluids, the authors find, as the mean specific heat of air under constant pressure,

·2390, from 0° to 100° Cent.

·2384, from 0° to 300° Cent.

XII. "Note on Nitro-glycerine." By A. W. Williamson, Ph.D., F.C.S., Professor of Practical Chemistry in University College. Communicated by Dr. Sharpey, Sec. R.S. Received June 15, 1854.

This compound is formed by acting upon glycerine with a mixture, in equal volumes, of concentrated nitric and sulphuric acids, the glycerine being added by a few drops at a time.

It is heavier than water, in which it is slightly soluble, and is soluble in alcohol and in ether.

From its proneness to decomposition in drying, even by the air-pump, a complete analysis could not be made, but a qualitative examination of the relative amounts of carbon and nitrogen gave the following results:—

	1.	2.	3.	4.
Volumes of mixed gases..................	101	91·5	99	97
Volumes of nitrogen not absorbed by potash..	32	30·5	34	33
Carbonic acid absorbed by potash..........	69	61	65	64

	1.	2.	3.	4.	5.
Mixed gases........	178	194	173	194	192
Nitrogen	61	66	58	65	65
CO_2	117	128	115	129	127

From these results the following formula was deduced:—

$$C_6\,H_8\,O_6+3NO_5=C_6\,{}^{H_5}_{3NO_4}\,O_6+3HO.$$

It would therefore appear that 3H are replaced by $3NO_4$.

On boiling this compound with concentrated solution of potash, it is decomposed into glycerine and nitrate of potash.

XIII. "On a New Phosphite of Ethyl, $3C_4 H_5O, PO_3$." By A. W. Williamson, Ph.D., &c. Communicated by Dr. Sharpey, Sec. R.S. Received June 15, 1854.

The following results were obtained by Mr. Railton in an investigation undertaken in connexion with the idea that the water of constitution discovered by Wurtz may be conceived as basic. The processes for preparing the compound are thus described by Mr. Railton.

1st. When three atoms of absolute alcohol are acted upon by one atom of PCl_3, this compound is formed. The alcohol is introduced into a retort which is connected with an apparatus for upward distillation, and the retort is surrounded with a freezing mixture. The terchloride is then added drop by drop, the whole is then gently heated for some time, the vapour being allowed to run back into the retort. It is now distilled and the portion which comes off between 140° C. and 196° C. collected and redistilled, that portion being preserved which boils between 188° and 191° C. The quantity of pure ether obtained by this process was not large, and there was left in the retort a considerable amount of PO_3 and other products, which on further heating evolved inflammable phosphuretted hydrogen.

2nd. This ether is obtained with the greatest facility from ethylate of soda and terchloride of phosphorus.

I introduce into a thirty ounce stoppered retort about a pint of ether, which must be perfectly free from alcohol and from water. The ethylate of soda is then added, and as much PCl_3 is taken as is necessary to form chloride of sodium and phosphite of ethyl. The ether is absolutely necessary, for without it, the action of the PCl_3 is so violent, as to set fire to the ethylate.

The PCl_3 is introduced into the mixture of ether and ethylate of soda through a long funnel, which is drawn to an extremely fine point, by which means it enters drop by drop into the mixture, thus avoiding the violent action which otherwise occurs.

The retort should be kept quite cool and frequently shaken. If these precautions are neglected considerable loss is experienced.

When the whole of the PCl_3 has been added, the ether is distilled off by a water-bath. The retort is then transferred to an oil-bath

which is gradually heated up to about 240° C. The whole of the distillate obtained by the oil-bath is collected in a dry receiver, and as it is prone to decomposition if distilled in air, it is distilled in an atmosphere of hydrogen, the portion which comes off at 188° C. is the phosphite of ethyl. I may here notice the remarkable fact, that this substance has two boiling points, as doubtless have many other bodies, if distilled under similar circumstances. In air it boils at 191° C. while, as I said before, it boils in hydrogen at 188° C. Its specific gravity is 1·075.

3rd. The reaction which occurs on the formation of this ether may be represented by the following formula:—

$$3NaO, C_4H_5O + PCl_3 = 3NaCl + 3C_4H_5O, PO_3.$$

The carbon and hydrogen were estimated in the usual manner by oxide of copper, the phosphorus as follows. A weighed portion of the ether was introduced into a twelve ounce stoppered bottle; concentrated nitric acid was poured upon it, and the bottle allowed to stand in a warm place, loosely stopped, for several days. When nitrous fumes no longer appeared, the oxidation of the phosphorous acid was deemed to be complete. The acid liquid was then saturated with ammonia, some chloride of ammonium and sulphate of magnesia then added, and the mixture well shaken. It was allowed to stand for some time, when a precipitate of phosphate of magnesia and ammonia was formed; this was washed, dried, and ignited, and the amount of phosphorus calculated from the result. These are the results.

Grms.		CO_2		HO	$2MgO, PO_5$
·2405	ether gave	·3784	and	·1920	,,
·5115	,,	·8047	and	·4155	,,
·4513	,,	,,		,,	·302
·4110	,,	,,		,,	·278

From these results the following per centages are calculated.

	Required.	Found.	
C_{12}	43·11	42·91	42·89
H_{15}	8·98	8·87	9·03
P	19·16	18·92	19·10
O_6	28·75	29·30	28·98
	100	100	100

These results being satisfactory as regards the formula, the density of the vapour was then ascertained and found to be in strict accordance with theory. The method of taking the vapour densities of bodies liable to oxidation was described by me about twelve months ago, in the Chemical Society's Quarterly Journal. It was used in the following experiments.

1st. Weight of globe filled with air at 53° F. and 30·2 in. barometer, 188·213 grs.

Weight of empty globe, 186·313 grs.

Weight of globe and vapour at 521° F. and 30·3 in. barometer, 192·387 grs.

Capacity of globe at 60° F., 6·00 cub. in.

Residual hydrogen ·05 cub. in.

Capacity of globe at 521° F., 6·40 cub. in.

Six cubic inches of air at 53° F. and 30·2 in. barometer, become at 60° F. and 30 in. barometer 6·12 cub. in., and weigh 1·90 grs.

·05 cub. in. hydrogen at 60° F. become ·094 cub. in. at 521° F., and weigh ·002 grs.

6·40 − ·094 = 6·306 cub. in. vapour at 521° F., which at 60° F. and 30 in. barometer = 3·376 cub. in.

Hence 192·387 − ·002 = 192·385 − 186·313 = 6·072 grains, the weight of 3·376 cub. in. vapour.

100 cubic inches . . = 179·86 grs.
100 cubic inches air = 31·01 grs.

The density is therefore 5·800, from which it appears that its combining measure is four volumes.

Density by calculation = 5·763.

A second experiment gave 5·877.

This substance has a highly offensive odour, it burns with a bluish white flame, is soluble in water, alcohol, and ether, and is slowly decomposed in contact with air.

On boiling phosphite of ethyl with concentrated solution of baryta, in water, it is decomposed into alcohol and a salt which varies according to the amount of baryta used. If one atom of the ether be treated with one of baryta, a crystallized salt is produced on evaporation, the carbon and hydrogen in which are, according to an analysis I have just completed.

	Found.	Required.
Carbon	20·354	24·158
Hydrogen ..	5·356	5·050
Baryta	36·880	37·090

In that marked 'required' I have supposed the salt to bear the following formula and to be completely anhydrous, $2C_4H_5O$, BaO, PO_3, but if we suppose that four atoms of water are present in the salt analyzed, the relation will stand thus

Found.	Required.
20·354	20·453
5·356	5·540

The formula would then be $2C_4H_5O$, BaO, $PO_3 + 4HO$.

When two atoms of baryta are made to act upon one atom of the ether, a salt is obtained which does not crystallize, and it may be evaporated in air without sensible decomposition. This salt is perfectly neutral to test paper; when dry it is a white friable deliquescent mass, the formula of which will be C_4H_5O, 2BaO, PO_3. If an excess of baryta is used, a white salt is thrown down on boiling, which I suppose to be HO, 2BaO, PO_3.

I have prepared another compound with three equivalents of amyle. This was obtained from amylate of soda by an analogous process to that described for the phosphite of ethyl.

Analysis has pointed out the formula $3C_{10}H_{11}O$, PO_3. Like phosphite of ethyl it is easily decomposed on being heated in air; heated in hydrogen it is more stable and then boils at 236° C. It is soluble in ether and in alcohol, but only slightly soluble in water.

XIV. "On some new derivatives of Chloroform." By A. W. Williamson, Ph.D., &c. Communicated by Dr. Sharpey, Sec. R.S. Received June 15, 1854.

According to the results of recent researches in the constitution of salts and the methods thence introduced of explaining chemical reactions, it is equally correct to represent such a reaction as that of hydrochloric acid on hydrate of potash, as consisting in an exchange of hydrogen of the one for potassium of the other, or of chlorine in one for peroxide of hydrogen in the other. In Mr. Kay's researches as described in the following brief outline, this notion has obtained very striking illustration; for he has obtained a peculiar body in which the chlorine of chloroform is replaced by peroxide of ethyle by the action of chloroform on three atoms of ethylate of sodium, which product may be equally well conceived to be a body in which the hydrogen of three atoms of alcohol is replaced by the tribasic radical of chloroform.

According to the older theories of the capacity of saturation of salts, this compound would contain a tribasic modification of formic acid, for it has the same relation to formic ether as a so-called tribasic phosphate has to a monobasic one.

To one equivalent of chloroform were added, by degrees, three equivalents of dry and powdered ethylate of soda, a violent action taking place with the evolution of much heat; the liquid was entirely distilled from the residue (chloride of sodium) by means of an oil-bath, and then subjected to a series of fractional distillations, which yielded a small distillate between 50° and 60° C., smelling strongly of vinous ether, a large distillate (about three-fourths of the whole) between 77° and 78° C., which was chiefly alcohol, and another small distillate (about one-sixth) between 145° and 145·3° C.

The distillates obtained by the above process, except that of alcohol, being small, the following modification was adopted.

Sodium was dissolved in absolute alcohol until the action became feeble, chloroform was then added, care being taken to keep the liquid alkaline; more sodium was then added, and the process repeated several times, until the chloride of sodium precipitated became very bulky. The liquid was then distilled off and chloroform

added to the residue, and also distilled off. To this first distillate sodium was again added, and treated with the last distillate instead of pure chloroform, the same precautions being used as before. This method gave similar distillates, and in about the same proportion as that first used; the highest distillate however boiled constantly at 146° instead of at 145·3° C.

This compound which boils at 145° to 146° C. is a colourless limpid liquid, only slightly soluble in water, having a strongly aromatic odour, readily inflammable and burning without much smoke; its specific gravity is ·8964; it remained liquid at 0° F.

Several analyses made of this body agree in giving to it the formula $C_7 H_{16} O_3$, which would also be the empirical formula of a tribasic formic-ether; the density of its vapour also corresponds very closely with the same formula.

Pentachloride of phosphorus added to a portion of the compound produced a heavy liquid having the odour of chloroform.

A small quantity of the body was dissolved in alcohol, distilled upwards for two or three hours with solid hydrate of potash and then distilled off; the residue was next dissolved in water and made exactly neutral by hydrochloric acid, filtered to remove the turbidity, and then a few drops of chloride of mercury added; after a little time and by the application of heat, a very slight precipitate of subchloride of mercury was formed; also the colour of sesquichloride of iron was a little darkened by another portion of the solution, thus showing that the action of potash on the compound had produced formic acid, but in very small quantity.

An equivalent of dry hydrochloric acid was passed into a portion of the compound; the gas was wholly absorbed, a considerable amount of heat being evolved and the liquid assuming a brownish colour; the liquid after the absorption of the gas still remained perfectly neutral. It was next distilled with the thermometer: it began to boil at 20° C. and rose gradually to 100°; it was collected in three portions, the first (about one-sixth of the whole) passing over between 20° and 50°, the second (about one-third) between 50° and 68°, the third (one-half) between 68° and 100°. I was unable to carry these distillations further in consequence of the small quantity of the liquid available.

Two equivalents of dry hydrochloric acid were passed into a larger quantity of the compound; towards the close the gas was ab-

sorbed less freely, a portion passing through; after this treatment, the liquid fumed and was highly acid; it was distilled upwards for some time by which a portion of free hydrochloric acid was expelled, and then distilled fractionally; about one-third came over between 56° and 60° C., one-fourth between 60° and 70°, one-sixth between 70° and 80°, and the remainder (about one-fourth) between 80° and 88°. To the lowest distillate about an equal bulk of water was added; the substance floated on the surface and seemed to be little, if at all dissolved by the water; a sufficient quantity of carbonate of soda was next added to neutralize the free acid, and the liquid pipetted from the water, it was then distilled upwards for some time with dry chloride of calcium, and afterwards distilled off; this distillate was found to boil constantly at 55·5° C. An analysis made of this body agrees closely with the formula $C_6 H_{14} O_5$.

The distillate which came over between 60° and 70° after being treated in the same way as the lower distillate, also yielded a liquid which boiled at 56° C.

As both methods hitherto used for the purpose of obtaining the body $C_7 H_{16} O_3$ afforded only small quantities, the treatment of chloroform with an alcoholic solution of potash was tried; for this purpose 12 oz. of solid hydrate of potash and 20 oz. of quicklime were added to about three pints of absolute alcohol, and the alcohol distilled upwards for six or seven hours; 6 oz. of chloroform were then added gradually, the upward distillation being continued about two hours longer; the liquid was next distilled off to dryness by means of an oil-bath, and submitted to fractional distillation; by this method a much larger quantity of the compound was obtained than by the former processes; it was found to boil constantly at 146° C., and its analysis agreed almost exactly with the formula. In this process the lowest distillate had the same smell of vinous ether which was before observed in the other methods.

An attempt was made to produce the intermediate compounds $C H Cl_2$, AeO, and $C H Cl$, 2AeO, by adding dry and powdered ethylate of soda very gradually to a large excess of chloroform; but the liquid after being separated from the precipitate, was found, on distilling fractionally, to resolve itself into chloroform, alcohol, and the body ($C_7 H_{16} O_3$) already obtained, the presence of no other substance being observable.

With a view of obtaining a compound analogous to the body $C_7 H_{16} O_3$, in which amyle should be introduced instead of ethyle, dry amylate of soda was prepared, to three equivalents of which one equivalent of chloroform was added, the liquid separated from the precipitate and then distilled fractionally; a large proportion of fusil-oil was obtained, together with a small proportion of a body which boiled at a high temperature,—from 260° to 290° C., but chiefly from 260° to 270°; the purification of this substance was not carried further, as at each distillation a considerable portion was decomposed even in an atmosphere of hydrogen, the small quantity of the liquid available precluding any more attempts at distillation.

June 15, 1854. (Continued.)

The EARL of ROSSE, President, in the Chair.

The following papers were read:—

XV. "On the Structure of certain Microscopic Test-objects, and their Action on the Transmitted Rays of Light." By CHARLES BROOKE, M.A., F.R.S., Surgeon of the Westminster Hospital. Received June 1, 1854.

In order to arrive at any satisfactory conclusions regarding the action of any transparent medium on light, it is necessary to form some definite conceptions regarding the external form and internal structure of the medium. This observation appears to apply in full force to microscopic test-objects; and for the purposes of the present inquiry it will suffice to limit our observations to the structure of two well-known test-objects, the scales of *Podura plumbea*, and the siliceous loricæ or valves of the genus *Pleurosigma*, freed from organic matter: the former of these is commonly adopted as the test of the *defining* power of an achromatic object-glass, and the several species of the latter as the tests of the *penetrating* or *separating* power as it has been termed. The defining power depends only on the due correction of chromatic and spherical aberrations, so that the image of any point of an object formed on the retina may not overlap and confuse the images of adjacent points; this correction is never theoretically perfect, since there will always be residual terms in the general expression for the aberration, whatever practicable number of surfaces we may introduce as arbitrary constants; but it is practically perfect, when the residual error is a quantity less than that which the eye can appreciate. The separation of the markings of the Pleurosigmata and other analogous objects, is found

to depend on good defining power associated with large angle of aperture.

The Podura scale appears to be a compound structure, consisting of a very delicate transparent lamina or membrane, covered with an imbricated arrangement of epithelial plates, the length of which is six or eight times their breadth, somewhat resembling the tiles on a roof, or the long pile of some kinds of plush. This structure may be readily shown by putting a live Podura into a small test-tube, and inverting it on a glass slide; the insect should then be allowed for some time to leap and run about in the confined space. By this means the scales will be freely deposited on the glass, and being subsequently trodden on by the insect, several will be found, from which the epithelial plates have been partially rubbed off, and at the margin of the undisturbed portion, the form and position of the plates may be readily recognized. This structure appears to be rendered most evident by mounting the scales thus obtained in Canada balsam, and illuminating them by means of Wenham's parabolic reflector. The structure may also be very clearly recognized when the scale is seen as an opake object under a Ross's $\frac{1}{12}$th (specially adjusted for uncovered objects), illuminated by a combination of the parabola and a flat Lieberkuhn, as the writer has elsewhere described*. The underside of the scale thus appears as a smooth glistening surface with very slight markings, corresponding probably to the points of insertion of the plates on the contrary side. The minuteness and close proximity of the epithelial plates will readily account for their being a good test of *definition*, while their prominence renders them independent of the *separating* power due to large angle of aperture.

The structure of the second class of test-objects above mentioned differs entirely from that above described; it will suffice for the present purpose to notice the valves of three species only of the genus *Pleurosigma*, which, as arranged in the order of easy visibility, are, *P. formosum*, *P. hippocampus*, *P. angulatum*.

These appear to consist of a lamina of homogeneous transparent silex, studded with rounded knobs or protuberances, which, in *P. formosum* and *P. angulatum*, are arranged like a tier of round shot in a triangular pile, andi n *hippocampus*, like a similar tier in a qua-

* See British Association Reports for 1850.

drangular pile, as has frequently been described; and the visibility of these projections is probably proportional to their convexity. The "dots" have by some been supposed to be depressions; this however is clearly not the case, as fracture is invariably observed to take place *between* the rows of dots, and not *through* them, as would naturally occur if the dots were depressions, and consequently the substance thinner there than elsewhere.

This in fact is always observed to take place in the siliceous loricæ of some of the border tribes that occupy a sort of neutral, and not yet undisputed, ground between the confines of the animal and vegetable kingdoms; as for example the *Isthmia*, which possesses a reticulated structure, with depressions between the meshes, somewhat analogous to that which would result from pasting together bobbinnet and tissue paper.

The valves of *P. angulatum* and other similar objects have been by some writers* supposed to be made up of two substances possessing different degrees of refractive power; but this hypothesis is purely gratuitous, since the observed phenomena will naturally result from a series of rounded or lenticular protuberances of one homogeneous substance. Moreover, if the centres of the markings were centres of greatest density, if in fact the structure were at all analogous to that of the crystalline lens, it is difficult to conceive why the oblique rays only should be visibly affected. When *P. hippocampus* or *P. formosum* is illuminated by a Gillett's condenser, with a central stop placed under the lenses, and viewed by a quarter-inch object-glass of 70° aperture, both being accurately adjusted, we may observe in succession, as the object-glass approaches the object, first a series of well-defined bright dots; secondly, a series of dark dots replacing these; and thirdly, the latter are again replaced by bright dots, not however as well defined as the first series. A similar succession of bright, dark, and bright points may be observed in the centre of the markings of some species of *Coscinodiscus* from Bermuda.

These appearances would result if a thin plate of glass were studded with minute, equal and equidistant plano-convex lenses, the foci of which would necessarily lie in the same plane. If the focal surface or plane of vision of the object-glass be made to coincide with this

* Vide Quarterly Journal of Microscopical Science, No. V. pp. 9, 10.

plane, a series of bright points would result from the accumulation of the light falling on each lens. If the plane of vision be next made to coincide with the surfaces of the lenses, these points would appear dark, in consequence of the rays being refracted towards points *now* out of focus. Lastly, if the plane of vision be made to coincide with the plane *beneath* the lenses that contains their several foci, so that each lens may be, as it were, combined with the object-glass, then a second series of bright points will result from the accumulation of the rays transmitted at those points. Moreover, as all rays capable of entering the object-glass are concerned in the formation of the second series of bright focal points, whereas the first series are formed by the rays of a conical shell of light only, it is evident that the circle of least confusion must be much less, and therefore the bright points better defined, in the first than in the last series.

If the supposed lenses were of small convexity, it is evident that the course of the more oblique rays only would be sensibly influenced; hence probably the structure of *P. angulatum* is recognized only by object-glasses of large angular apertures, which are capable of admitting very oblique rays.

The writer has recently, in an address to the members of the Royal Institution, proposed to explain the extreme darkness of the dots, under certain conditions of focus and illumination, by the hypothesis that some of the oblique rays are thrown out of the field by internal reflexion, being incident at the upper surface at an angle too large for emergence; but this does not appear to invalidate the present hypothesis respecting the course of the transmitted rays.

It does not appear to be desirable that objects should be illuminated by an entire, or, as it may be termed, a *solid* cone of light of much larger angle than that of the object-glass. The extinction of an object by excess of illumination may be well illustrated by viewing with a one-inch object-glass the *Isthmia* illuminated by Gillett's condenser. When this is in focus, and its full aperture open, the markings above described are wholly invisible; but as the aperture is successively diminished by the revolving diaphragm, the object becomes more and more distinct, and is perfectly defined when the aperture of the illuminating pencil is reduced to about 20°. The same point may be attained, although with much sacrifice of definition, by gradually depressing the condenser, so that the rays may

diverge before they reach the object; and it may be remarked generally that the definition of objects is always most perfect, when an illuminating pencil of suitable form is accurately adjusted to focus, that is, so that the source of light and the plane of vision may be conjugate foci of the illuminator. If an object-glass of 120° aperture or upwards be used as an illuminator, the markings of Diatomaceæ will be scarcely distinguishable, with any object-glass; the glare of the central rays overpowering the effects of structure on those that are more oblique.

XVI. "On the Constitution of Coal-tar Creosote." By Professor WILLIAMSON. Communicated by Dr. SHARPEY, Sec. R.S. Received June 15, 1854.

For some years past it has been a debated question among chemists, whether the peculiar body originally described by Reichenbach as creosote, and subsequently analysed by Ettling and others, has any real existence, or whether the properties which were attributed to it are not to be more correctly ascribed to the hydrate of phenyl, which can be obtained in a state of great purity from at least one sort of commercial creosote by mere distillation, and which possesses in an eminent degree the antiseptic properties for which creosote is remarkable.

With a view of obtaining some light on this question, Mr. Fairlie undertook, in the laboratory of University College, an investigation of the portions of coal-tar creosote which boil higher than the hydrate of phenyl. The result of his experiments has been to show that a body homologous to hydrate of phenyl may be obtained from the crude creosote, in fact the next term of the series above hydrate of phenyl itself. Some qualities of commercial creosote contain a greater quantity of this *hydrate of cresyl* (as it may be termed) than others; and it is most advantageously prepared from those portions which in the first distillation come over between 200° Cent. and 220°. After a great number of fractional distillations, a colourless, highly dispersive liquid is obtained, boiling at 203° Cent., and possessing the composition represented by the formula $C_{14}H_8O_2$.

This hydrate of cresyl resembles the corresponding phenyl compound in most of its properties; but it may be easily distinguished from that compound by its almost complete insolubility in aqueous ammonia.

When gradually mixed with sulphuric acid, it becomes of a beautiful rose-colour, and gives rise to sulpho-cresylic acid.

The action of nitric acid upon hydrate of cresyl is very violent, and almost explosive if the acid is used in a concentrated state and at so high a temperature as the common atmospheric; even very dilute nitric acid transforms the compound into a brown tarry mass from which no definite substance can be extracted. By cooling some nitric acid in a frigorific mixture and allowing some similarly cooled hydrate to fall into it drop by drop and with constant agitation, a red-coloured solution was obtained, which by dilution with water and subsequent neutralization by potash yielded a crop of short needle-shaped crystals of an orange-red colour, and possessing a greater solubility in water than the salt of carbazotic acid. This salt was found by analysis to possess the composition of a homologue of carbazotate of potash; so that it is the potash salt of a hydrate of cresyl in which three atoms of hydrogen are replaced by hyponitric acid,

$$C_{14}\begin{matrix}H_4\\(NO_4)_3\end{matrix}O, KO.$$

The same acid was obtained by the action of nitric acid upon an alcoholic solution of the hydrate containing urea; but in attempting to repeat this experiment on a larger scale the mixture became hot, and the whole of the substance was destroyed with almost explosive violence.

When treated with pentachloride of phosphorus this hydrate of cresyl is decomposed in like manner with the hydrate of phenyl, as described by Mr. Scrugham, yielding a chloride of cresyl and a phosphate of the same radical.

By the action of this phosphate in an alcoholic solution of acetate of potash, a peculiar oleaginous body is obtained possessing an odour entirely different from that of the hydrate, and decomposable by potash with production of acetate and cresylate.

A similar reaction ensues when the phosphate is distilled with ethylate of potash, and a cresylate of ethyl is thus obtained.

In the numerous distillations which were performed for the purification of the hydrate of cresyl, some circumstances were observed which led to a suspicion that the body undergoes a change of composition, either through the distillation itself, or by some influences accompanying it. These circumstances were,—1st. A tarry residue, from a liquid which when introduced into the retort was perfectly colourless. 2nd. The formation of a small quantity of water in the commencement of such a distillation, though none was contained in the substance used. 3rd. The gradual lowering of the boiling-point of the whole liquid by a great number of distillations. These facts, taken in conjunction, naturally suggested that the oxygen of the air contained in the retort might act upon the substance, and thus gradually reduce it to hydrate of phenyl.

In order to test the correctness of this hypothesis, the atmospheric air was expelled from the distilling apparatus by dry hydrogen gas, and the distillation performed in a pure atmosphere of this gas. A great number of distillations performed in this manner were at exactly the same temperature, and all the other anomalies were simultaneously removed. It was however found that the liquid always boiled at a lower temperature in hydrogen than in atmospheric air, the difference being about 2° Cent., and this without any alteration of the pressure on the surface of the boiling liquid. A similar fact was noticed in the distillation of hydrate of phenyl, and also of some other liquids.

XVII. "On the Formation of Powers from Arithmetical Progressions." By C. Wheatstone, Esq., F.R.S. Received June 15, 1854.

The same sum n^a may be formed by the addition of an arithmetical progression of n terms in various ways. Hence we are enabled to construct a great variety of triangular arrangements of arithmetical progressions, the sums of which are the natural series of square, cube and other powers of numbers. Among these there are several which render evident some remarkable relations.

Each of the following triangles is formed of a series of arithmetical progressions, the number of terms increasing successively by unity.

The first term of an arithmetical progression of n terms having a common difference δ, and whose sum is n^a, is equal to

$$n^{(a-1)}+\frac{\delta}{2}(1-n).$$

§ 1. SQUARE NUMBERS.

If $S=n^2$, the first term $=n+\frac{\delta}{2}(1-n)$.

A.

Every square n^2 is the sum of an arithmetical progression of n terms, the first term of which is unity and the difference 2.

$$\begin{array}{rl}
1 \ldots\ldots\ldots\ldots & =1^2 \\
1+3 \ldots\ldots\ldots\ldots & =2^2 \\
1+3+5 \ldots\ldots\ldots\ldots & =3^2 \\
1+3+5+7 \ldots\ldots\ldots\ldots & =4^2 \\
1+3+5+7+9 \ldots\ldots\ldots\ldots & =5^2 \\
1+3+5+7+9+11 \ldots\ldots\ldots\ldots & =6^2 \\
1+3+5+7+9+11+13 \ldots\ldots\ldots\ldots & =7^2
\end{array}$$

Thus, every square number is formed by the addition of a series of odd numbers commencing with unity; a result universally known.

The difference of any two squares is either an odd number, or the sum of consecutive odd numbers.

Each series may be resolved into two others consisting of alternate odd numbers, the respective sums of which are two adjacent triangular numbers, the addition of which it is well known forms a square. *Ex.*:

$$\begin{array}{r}
1+5+9+13=28 \\
3+7+11=\underline{21} \\
49=7^2
\end{array}$$

B.

Every square n^2 is the sum of an arithmetical progression of n terms, the first term of which is $\frac{n+1}{2}$, and the common difference 1.

$$
\begin{aligned}
1 \ \ldots\ldots\ldots\ldots\ldots\ldots &= 1^2 \\
1\tfrac{1}{2}+2\tfrac{1}{2} \ \ldots\ldots\ldots\ldots\ldots &= 2^2 \\
2+3+4 \ \ldots\ldots\ldots\ldots\ldots &= 3^2 \\
2\tfrac{1}{2}+3\tfrac{1}{2}+4\tfrac{1}{2}+5\tfrac{1}{2} \ \ldots\ldots\ldots\ldots &= 4^2 \\
3+4+5+6+7 \ \ldots\ldots\ldots\ldots &= 5^2 \\
3\tfrac{1}{2}+4\tfrac{1}{2}+5\tfrac{1}{2}+6\tfrac{1}{2}+7\tfrac{1}{2}+8\tfrac{1}{2} \ \ldots\ldots\ldots &= 6^2 \\
4+5+6+7+8+9+10 \ \ldots\ldots &= 7^2
\end{aligned}
$$

This arrangement renders evident that every square of an odd number is the sum of as many consecutive natural numbers as the root has units.

Every square of an odd number is the difference between two triangular numbers the bases of which are respectively $(3n+1)$ and n. For, the sum of any series of natural numbers is the difference of two series of natural numbers commencing with unity; and since, as it is shown above, every square of an odd number is the sum of a series of natural numbers, it is also the difference between two triangular numbers.

It is also evident that series, the sums of which are squares of odd numbers, may be so taken that, when placed in succession, they will form an uninterrupted progression of natural numbers commencing with unity, the sum of which is a triangular number;

$$(1)+(2+3+4)+(5+6+7+8+9+10+11+12+13)\ldots\&c.=$$

$$(1^2+3^2+9^2+27^2\ldots\ldots+(3^n)^2)=$$

a triangular number the base of which is the series

$$(1+3+9+27\ldots\ldots+3^n).$$

§ 2. CUBE NUMBERS.

If $S=n^3$, the first term $=n^2+\frac{\delta}{2}(1-n)$.

C.

Every cube n^3 is the sum of an arithmetical progression of n terms, the first term of which is unity, and the difference $2(n+1)$.

$$
\begin{array}{rr}
1 \ldots\ldots\ldots\ldots\ldots\ldots & =1^3 \\
1+7 \ldots\ldots\ldots\ldots\ldots\ldots & =2^3 \\
1+9+17 \ldots\ldots\ldots\ldots\ldots & =3^3 \\
1+11+21+31 \ldots\ldots\ldots\ldots & =4^3 \\
1+13+25+37+49 \ldots\ldots\ldots & =5^3 \\
1+15+29+43+57+71 \ldots\ldots & =6^3 \\
1+17+33+49+65+81+97 \ldots\ldots & =7^3
\end{array}
$$

D.

Every cube n^3 is the sum of an arithmetical progression of n terms, the first term of which is the root n, and the difference $2n$.

$$
\begin{array}{rr}
1 \ldots\ldots\ldots\ldots\ldots\ldots & =1^3 \\
2+6 \ldots\ldots\ldots\ldots\ldots\ldots & =2^3 \\
3+9+15 \ldots\ldots\ldots\ldots\ldots & =3^3 \\
4+12+20+28 \ldots\ldots\ldots\ldots & =4^3 \\
5+15+25+35+45 \ldots\ldots\ldots & =5^3 \\
6+18+30+42+54+66 \ldots\ldots & =6^3 \\
7+21+35+49+63+77+91 \ldots\ldots & =7^3
\end{array}
$$

The last terms of these series are the alternate triangular numbers. If they be respectively divided by the first terms, the quotients will be the series of odd numbers.

E.

Every cube n^3 is the sum of an arithmetical progression of n terms, the first term of which is (n^2-n+1), and the difference 2.

$$
\begin{array}{rr}
1 \ldots\ldots\ldots\ldots\ldots\ldots & =1^3 \\
3+5 \ldots\ldots\ldots\ldots\ldots\ldots & =2^3 \\
7+9+11 \ldots\ldots\ldots\ldots\ldots & =3^3 \\
13+15+17+19 \ldots\ldots\ldots\ldots & =4^3 \\
21+23+25+27+29 \ldots\ldots\ldots & =5^3 \\
31+33+35+37+39+41 \ldots\ldots & =6^3 \\
43+45+47+49+51+53+55 \ldots\ldots & =7^3
\end{array}
$$

This, it will be observed, is a triangular arrangement of the uneven numbers in their regular order.

Every cube is the sum of as many consecutive odd numbers as there are units in the root*.

The known theorem, that the sum of the cubes of any succession of the natural numbers commencing with unity is equal to the square of the sum of the roots, or, in other words, to the square of the corresponding triangular number, is an immediate consequence of the above.

$$(1^3+2^3+3^3+4^3\ldots\ldots+n^3)=(1+2+3+4\ldots+n)^2=\left(\frac{n^2+n}{2}\right)^2.$$

The sum of any series of odd numbers commencing with unity being equal to the square of the number of terms (A.), the sum of the numbers in any triangle formed as above is necessarily equal to the square of a triangular number. It is also easy to see that each cube is the difference between the squares of two consecutive triangular numbers; and, that the difference between the squares of any two triangular numbers whatever is the sum of consecutive cubes. The following equations have been found by ascertaining what differences of the squares of two triangular numbers are equal to single cubes:—

$$3^3+4^3+5^3=6^3$$
$$11^3+12^3+13^3+14^3=20^3.$$

F.

Every cube n^3 is the sum of an arithmetical progression of n terms, the first term of which is a triangular number $\frac{n^2+n}{2}$, and the difference $=n$.

$$\begin{aligned}
1 \quad &\ldots\ldots\ldots\ldots\ldots\ldots =1^3\\
3+5 \quad &\ldots\ldots\ldots\ldots\ldots =2^3\\
6+9+12 \quad &\ldots\ldots\ldots\ldots =3^3\\
10+14+18+22 \quad &\ldots\ldots\ldots =4^3\\
15+20+25+30+35 \quad &\ldots\ldots =5^3\\
21+27+33+39+45+51 \quad &\ldots\ldots =6^3\\
28+35+42+49+56+63+70 \quad &\ldots =7^3
\end{aligned}$$

* Since the present note was communicated to the Royal Society, I have found that this relation has been already noticed by Count d'Adhémar (Comptes Rendus, tom. xxiii. p. 501). Cauchy observes, "quoiqu'elle puisse, comme on le voit, se déduire des principes déjà connus, toutefois, elle est assez curieuse et très simple."

Each number contained in this triangle is itself the sum of an arithmetical progression of n terms. Thus, taking the fifth row for example :—

$$\begin{array}{r}
1+2+3+4+5= 15 \\
2+3+4+5+6= 20 \\
3+4+5+6+7= 25 \\
4+5+6+7+8= 30 \\
5+6+7+8+9= \underline{35} \\
125=5^3
\end{array}$$

The sum of all the numbers contained in a square thus formed is equal to the cube of the number which occupies the upper right-hand and lower left-hand corners. The sum of the numbers in either of the diagonals is the corresponding square, and in the case of the odd numbers the sum of the middle horizontal or vertical line is also the square.

This last-mentioned relation was pointed out by Lichtenberg*, who stated the theorem thus :—If a be a whole number, and A be the sum of all the natural numbers from 1 to a, then :

$$a^3=A+(A+a)+(A+2a)+(A+3a)\ldots\ldots+(A+[a-1]a).$$

G.

Every cube n^3 above 1 is the sum of an arithmetical progression of n terms, the first term of which is $(n-2)^2$, and the difference $=8$.

$$\begin{array}{rl}
0+8 \ \ldots\ldots\ldots\ldots\ldots & =2^3 \\
1+9+17 \ \ldots\ldots\ldots\ldots & =3^3 \\
4+12+20+28 \ \ldots\ldots\ldots\ldots & =4^3 \\
9+17+25+33+41 \ \ldots\ldots\ldots & =5^3 \\
16+24+32+40+48+56 \ \ldots\ldots & =6^3 \\
25+33+41+49+57+65+73 \ \ldots & =7^3
\end{array}$$

Each progression of this triangle, consisting of an uneven number of terms, contains two consecutive odd square numbers.

An *uninterrupted* arithmetical progression commencing with unity and proceeding by the constant addition of 8, arranged in a triangular form, presents some curious results. 1st. The first terms of

* G. C. Lichtenberg's Vermischte Schriften, Band ix. p. 359. Göttingen 1806.

each line are the squares of the odd numbers in their regular sequence. 2nd. The sum of all the numbers in any two adjacent lines is the cube of an odd number.

$$\left.\begin{array}{r} 1 \;\ldots\ldots\ldots\ldots \\ 9+17 \;\ldots\ldots\ldots \\ 25+33+41 \;\ldots\ldots \\ 49+57+65+73 \;\ldots\ldots \\ 81+89+97+105+113 \;\ldots \\ 121+129+137+145+153+161 \end{array}\right. \begin{array}{l} \}\;\ldots\ldots = 3^3 \\ \}\;\ldots\ldots = 5^3 \\ \}\;\ldots\ldots = 7^3 \\ \}\;\ldots\ldots = 9^3 \\ \}\;\ldots\ldots = 11^3 \end{array}$$

It is evident from the preceding arrangement that

$$(2n+1)^2=1+8\left(\frac{n^2+n}{2}\right).$$

Thus any triangular number multiplied by 8 with 1 added is equal to the square of an odd number; or, any square of an uneven number minus 1 is divisible by 8, and the quotient is a triangular number.

§ 3.

Of the higher powers I will confine myself to one example.

H.

Every fourth power n^4 is the sum of an arithmetical progression of n terms, the first term of which is n^2, and the difference $2n^2$.

$$\begin{array}{rl} 1 \;\ldots\ldots\ldots\ldots\ldots & =1^4 \\ 4+12 \;\ldots\ldots\ldots\ldots & =2^4 \\ 9+27+45 \;\ldots\ldots\ldots & =3^4 \\ 16+48+80+112 \;\ldots\ldots\ldots & =4^4 \\ 25+75+125+150+225 \;\ldots\ldots & =5^4 \\ 36+108+180+252+324+396 \;\ldots & =6^4 \end{array}$$

This triangle consists of the progressions in (D.) multiplied respectively by n, or of those in (A.) multiplied by n^2.

XVIII. "On the Structure and Functions of the Rostellum in *Listera ovata*." By J. D. Hooker, M.D., F.R.S. Received June 15, 1854.

The author first gives an account of the form and structure of the rostellum of *Listera ovata*, and its relation and position to the anther and stigma. He finds that the rostellum is divided by parallel septa (at right angles to the plane of that organ) into a series of longitudinally elongated loculi, which gradually taper from the base upwards, and terminate at two opake cellular spots, one on each side of the apex of the rostellum, towards which latter the loculi also converge. When the flower is fully expanded, these loculi are distended with a viscid grumous fluid, full of chlorophyll granules. Their external walls, and the septa dividing them, are formed of a delicate, transparent tissue, which is cellular at the base and apex of the rostellum only.

Their grumous contents, when examined at the earliest period of development, present the appearance of opake club-shaped compressed bodies, with areolated surfaces; a form and appearance that may be restored at a later period by coagulating with alcohol.

At the period of impregnation the slightest irritation of the rostellum causes the sudden and forcible discharge of the contents of these loculi (through the rupture of the cellular tissue at the apex of the rostellum) and its protrusion in the form of two viscid glands, which coalesce into one, after which the rostellum rapidly collapses and contracts.

The pollen-masses, when freed from the anther-case, fall naturally upon the rostellum; they are retained there by their viscid gland-like contents, and, breaking up, the pollen-grains become (by the contraction of the rostellum) applied to the subjacent stigmatic surface.

The author adds remarks on the structure of the rostellum in allied genera of Orchideæ, and indicates some of the more important morphological changes to which that organ is subjected, in connection with the development of various appendages to the column and pollen in the same natural family.

XIX. "On the Immediate Principles of the Excrements of Man and Animals in the Healthy Condition." By WILLIAM MARCET, M.D. Communicated by F. MARCET, F.R.S. Received June 14, 1854.

The author describes a new method of extracting the immediate chemical constituents of the excrements of Man and animals, and gives an account of the substances obtained by its employment.

Healthy human fæces are boiled to exhaustion in alcohol. The residue is insoluble in ether, and yields to boiling water nothing but ammoniaco-magnesian phosphate. The strained alcoholic solution deposits, on standing, a sediment, from which it is decanted and then mixed with milk of lime. The subsiding lime is of a yellow-brown colour; it is dried on filtering-paper and treated with ether, cold or hot, and the solution thus obtained yields, on spontaneous evaporation, beautiful silky crystals, which are purified by solution in a mixture of alcohol and ether, repeated filtration through animal charcoal and recrystallization; they then appear in circular groups, have the form of acicular four-sided prisms, and polarize light very readily. This crystalline body the author proposes to call *Excretine*. It is very soluble in ether, cold or hot, but sparingly soluble in cold alcohol; its solution has a decided though weak alkaline reaction. It is insoluble in hot or cold water, and is not decomposed by dilute mineral acids. It fuses between 95° and 96° C., and at a higher temperature burns away without inorganic residue. When boiled with solution of potash it does not dissolve. As to its qualitative constitution, it is found to contain nitrogen and sulphur, though in small proportions; the products of its decomposition have not yet been investigated.

The author has in several cases observed the excretine to crystallize directly in the alcoholic solution of fæces before the addition of lime, and has scarcely any doubt that it exists for the most part in a free state in the excrements, and constitutes one of their immediate principles. As to its source, he observes that it appeared in excess when a considerable quantity of beef had been taken, and in less than the usual quantity in a case of diarrhœa attended with loss of appetite; but none could be directly obtained from beef on

subjecting it to the same process of extraction as fæces. Neither could it be found in ox-bile, the urine, or the substance of the spleen. From the difficulty of obtaining the contents of the human small intestine in a healthy state, its presence or absence in that part of the alimentary canal has not yet been satisfactorily determined.

The lime precipitate, after having been thus thoroughly deprived of the excretine by ether, is next treated with hydrochloric acid, and water or alcohol, by which means margaric acid is extracted from it. The author is uncertain whether the margaric acid of the fæces is free or combined with excretine, but he is disposed to conclude that the neutral fats are decomposed in the intestinal canal and their acid set free. Not having been able to detect stearic acid in human evacuations, he supposes that what is contained in the fat of mutton or beef taken as food must be converted into margaric acid in its passage through the alimentary canal.

The lime precipitate, freed from excretine and dissolved in alcohol by means of hydrochloric acid, forms a dark port-wine-coloured solution, from which the margaric acid is deposited. On then adding water to the solution and concentrating it on the water-bath, a flaky colouring matter separates, which, being purified by solution in ether and washing with water, is obtained as a dark-brown or black amorphous substance, similar to the colouring matter of blood, and to that which Dr. Harley has lately extracted from urine.

The matters brought down with the lime having been thus extracted, the sediment which spontaneously subsides from the alcoholic solution of fæces before its treatment with the milk of lime, is next examined. This deposit appears to be complex in its nature; it has a strongly acid reaction, and presents under the microscope small oily globules, mixed sometimes with crystals of excretine and accompanied by a yellow amorphous matter. By boiling with alcohol and filtration, a residue remains which the author has not yet examined, and two substances are obtained from the filtrate. The first is deposited on cooling; when collected and dried on filtering-paper it has a granular character and is quite colourless; it is very sparingly soluble in ether, fuses by heat, and burns with a bright fuliginous flame, leaving a white residue consisting of phosphate of potash. The author has not yet been able satisfactorily to decide whether this is a pure immediate principle or not; he is inclined to consider

it as a combination of phosphate of potash and a pure organic substance. The filtered fluid, after separation of this matter, still contains a substance which he has called *Excretolic acid*. It is obtained by evaporating to dryness, extracting the residue with ether, adding to the ethereal solution alcohol and lime-water, and heating. The acid is precipitated in combination with lime, from which it is separated by means of sulphuric or hydrochloric acid and solution in ether. The ethereal solution, after being well washed with water to remove mineral acid, yields the pure excretolic acid on evaporation. This body is of an olive colour; it fuses between 25° and 26° C., and at a higher temperature burns without residue. It is insoluble in water and in a boiling solution of potash; very soluble in ether, sparingly soluble in cold alcohol, readily so in hot; its solutions having a marked acid reaction. The author is disposed to believe that in excrement it is combined in form of a salt, with excretine or a basic substance closely allied to it, which is obtained in the filtrate from which the excretolic acid is precipitated in combination with lime in the process of its purification.

The author failed to obtain evidence of the presence either of butyric or of lactic acid in the clear alcoholic solution of fæces filtered from the precipitate formed by the milk of lime. From the above investigation, therefore, it appears that healthy human excrements contain :—

1. A new organic substance, possessing an alkaline reaction, which the author names *Excretine*.

2. A fatty acid, having the properties of margaric acid, but not constantly present.

3. A colouring matter, similar to that of blood and urine.

4. A light granular substance, whose properties have not yet been sufficiently examined to admit of its being considered a pure substance.

5. An acid olive-coloured substance, of a fatty nature, named *Excretolic acid*.

6. No butyric acid and no lactic acid.

The fæces of various animals were submitted to the same process of analysis, with the following results :—

1. The excrements of carnivorous mammalia, viz. the Tiger, Leopard and Dog (fed on meat), contain a substance allied in its na-

ture to excretine, but not identical with it. They contain no excretine; they yield butyric acid, which is not present in human excrements.

2. The excrements of the Crocodile contain cholesterine and no uric acid, whilst those of the Boa yield uric acid and no cholesterine.

3. The fæces of herbivorous animals, viz. the Horse, Sheep, Dog (fed on bread), Wild Boar, Elephant, Deer and Monkey, contain no excretine, no butyric acid and no cholesterine.

XX. "On the Vine-Disease in the Port-wine Districts of the Alto-Douro, in April 1854. With a Supplementary Note on the Proposed Remedies for its Eradication." By Jos. James Forrester, Esq., F.R.G.S. Communicated by J. P. Gassiot, Esq., F.R.S. Received May 17, 1854.

In Portugal, where the vine-disease committed great ravages last year, no measures have as yet been adopted for ascertaining whether the disease is *radical*, or only superficial; or whether any *practical remedy* may be adopted in order to arrest the progress of the evil.

At Oporto, and in the north of Portugal, an opinion prevails—

"That the *Oïdium* is the *effect*, and not the *cause* of the epidemic.

"That the roots and the wood of the vines are diseased.

"That *sporules* of the *Oïdium* exist in the interior of the vine, and about its roots.

"That the obstruction to the ascent of the sap through the various ducts, originates in the roots.

"That black spots appear in the joints of the branches, indicating that disease exists throughout the body of the vine.

"That a new fungus has appeared on the vines, in the shape of small globules, containing carbonic acid.

And "that, although vegetation may continue for a while, the fruit will not ripen, and the vines will die in a couple of years from this date."

Considering that it would be of some importance to determine whether the disease has its origin in the roots or from external

causes, and with a hope that some practical cure for the diseased vines grown in the open air may be discovered, I record the results of my own observations of the progress of the vine-malady in the Alto-Douro.

The Port-wine District extends eight leagues west and east from the Serra do Marão (an elevation of 4400 feet* from the level of the sea) to the Quinta do Baleira, near Sam João da Pesqueira, and four leagues north and south, between Villa Real and the city of Lamego †.

The winter streams, tributaries to the Douro, on the right bank, are the Sermenha, Corgo, Ceira, Pinhão, and Tua; and, on the left, the Varoza, Temilobos, Tedo, Tavora, and Torto.

At Baleira, the Douro runs at an elevation of not more than 250 feet; whence some opinion may be formed of the nature and inequality of the country, and of the numerous abrupt mountain ridges, on the inclines of which the vines are grown. The Wine-Districts of the Alto-Douro form a long irregular basin, girt by the granite chains of the Tras-os-Montes and Beira; and this being for the most part of schist formation, and protected from the bleak winds, is particularly adapted for the cultivation of the vine. The strata of the margins of the Douro differ from the higher and middle grounds in character, " being composed of strong clays, more or less micaceous."

The extreme northern and southern boundaries—from the Serra do Marão to Favaios, and from the Serra do Monte Muro (near Lamego) to Sam João da Pesqueira — are undulating mountain plains of still heavier soil, and more suitable for the growth of firs than vines. In former years, this fact was clearly defined by the Royal Wine Company, who divided the districts into two, one being termed Feitoria (where the most superior wines were produced and classified for exportation), the other Ramo, where only very inferior wines, for the consumption of the country and for distillation, were produced to a small extent. Now, the two districts have become one; the plantations of pines on the heights and the corn-producing valleys having alike been converted into vineyards; the

* " Considerações geraes sobre a Constituição Geologicao do Alto-Douro." By Dr. J. P. Rebello. Porto, 1848.

† See map of the Wine-Districts of the Alto-Douro. By J. J. Forrester.

quantity, and not the *quality*, of the produce being the results sought by the wine-grower within this privileged demarcation.

One thousand vines generally produce a pipe of wine, and the total number of vines in the Port-wine Districts above described may be estimated at 90,000,000.

In the summer time, there is great scarcity of water throughout the district. The vineyards are for the most part situated on abrupt mountain slopes, the vines being planted on terraces, which are not appropriate for the cultivation of anything else. The vines are grown not higher than three feet from the ground, and are planted about six feet apart, supported with canes or stakes. The labour in the vineyards is performed by the natives of Gallicia, who visit the district three or four times a year in search of employment.

In July 1850, I first observed a blight on three or four vines, at a considerable distance from each other, in the Wine Districts. The general appearance of this blight to the naked eye greatly resembled that which appears on the peach-tree and the rose. The Douro farmers had often previously noticed a similar *po' branco* (white powder) on the vines.

In 1851 the season was favourable, and the vines (on which we had observed the blight in the previous year) were vigorous, and produced perfect fruit. The vintage of 1851, throughout the Alto-Douro, was excellent. In 1852 there was much wet and cold; the blight again appeared, and the vines were attacked to the extent of about one in fifteen hundred. The vintage of 1852 was of inferior quality; but no one ascribed the failure to any disease in the vine. From the autumn of 1852 until midsummer 1853, continued rain, sleet, hail, and bleak winds prevailed, and in 1853 there was no spring. In March of the same year the navigation of the Douro was impeded, and the bar rendered impassable on account of the floods; and in April and May of the same year, prayers were offered up in the churches throughout the Wine Districts for fine weather.

In March 1854, only half-cargoes could be brought down the river Douro, on account of the want of water, and rain was prayed for.

Early in June 1853, the heat became suddenly intense, and the vines had already burst forth with great vigour; whilst, in the middle of the same month, the nights became as cold as in winter.

In the most exposed situations the vines received the greatest shock; the circulation of the sap was evidently deranged, and their fruit withered as soon as it appeared. In some neighbouring vineyards, less exposed, the grapes grew no larger than peas; they were then suddenly covered with the blight (now designated the *Oïdium*), and in about three days became rotten.

On the inclines of the mountains on either bank of the river Douro, the waters had run off, and but little blight appeared. In the low and heavy grounds, the most sheltered from the winds, the waters remained stagnant; yet the fruit grew to its full size, and had come to maturity, when the new wood, leaves, and fruit were all, to a greater or less degree, covered with the *Oïdium*. The blight sometimes attacked entire vineyards, and at other times only partially affected one property, and then showed itself in others at a distance—intermediate estates being for the time wholly untouched.

It was in July 1853 that the existence of the disease in the vineyards of the Douro first attracted particular attention; but many vines betrayed no unhealthy symptoms until the fruit was nearly ripe. The upper part of the branches was first attacked. In some instances the woody part of the young branches was speckled with the *Oïdium*, while the bunches of fruit were apparently altogether free from it. In other instances, the grapes became touched with the disease immediately before the vintage, but the woody part of the branches betrayed no such symptoms. In some vines, which I supposed had altogether escaped the disease (and long after the fruit was gathered and the leaves had fallen off), blotches or stains, evidently the *mycelium* of the *Oïdium*, appeared on the wood.

The usual number of seeds in a black grape is two or three; but in the year 1853, in all instances the grapes, which at first promised abundance of wine, were found each to contain from three to five seeds.

Twenty-one baskets of grapes usually produce one pipe of wine; but in the year 1853, a pipe of wine was rarely obtained even from thirty baskets of grapes. From seven to nine pipes of ordinary wine generally give a pipe of brandy, 20 per cent. above British proof; but in the year 1853, from ten to twelve pipes of ordinary wine were required to give one pipe of brandy of that strength.

Wines, when properly made, should be trodden continuously for 36 hours in the *lagar* (an open stone vat), and remain there for 36 to 48 hours more, until the tumultuous fermentation be completed, when they should be run off into larger *tonels* (wooden vats, not tightly bunged), where the second fermentation will be completed about Christmas. In 1853, in situations where the disease most prevailed, the grapes fermented before they had been trodden more than twelve hours, when the wines were drawn off and passed into tonels, where brandy, as a precautionary measure, was given to them. The fermentation of these wines ceased altogether before the 15th October. In other situations, where the disease had not made progress, the grapes were sound; and, where they were properly crushed and fermented, they produced excellent wine, without the addition of brandy.

Wines, during their second fermentation, deposit a thick coating of argol on the sides of the tonels. In 1853 there was very little argol deposited; but the gross lees of the wine were in great demand, and sold for about 15*s*. per basket,—a sum which in former years might almost have purchased double the quantity of grapes.

In the same manner as the form and colour of the wood, leaves, and fruit of vines differ, so does their pith vary in appearance, according to the age of the wood or the quality of the vine. The pith in an old vine, when the sap is rising, graduates from a deep vandyke-brown colour to a pale yellow, the shade being always darker near the joints.

In April 1854, I rooted up many vines of different qualities, and in various situations, and I was unable then to detect any remarkable appearance in the interior of the vine different from what I had seen in other years after continued wet and cold weather; but the exterior of all the last year's branches bore palpable evidence of having been violently attacked with the *Oïdium*. Some vines had suffered more than others, and many of their vessels were evidently choked; but, in most instances, in cutting the vine longitudinally, this obstruction was found to have arisen either from wounds, bad pruning, or natural decay. I found no black spots at the joints of the branches; and, with the exception of the stains left by the disease of last year, the vines looked healthy and vigorous, throwing out strong shoots and promising an abundance of fruit.

Towards the end of April 1854, much rain fell in the district; the easterly winds destroyed the young branches; and in the most exposed situations and heavy soils the *Oïdium* again made its appearance.

In 1853, the disease attacked the vines bodily, and almost simultaneously: whereas, in 1854, the *Oïdium* appears to be creeping out of the skin of the last year's wood, and insidiously to extend itself over the branches.

The globules (to which allusion has been made above) cover the young shoots. I have been familiar with these for twenty-three years past, and the Douro farmers call them the "perspiration" of the vine. They do not indicate disease, whereas the smallest possible quantity of the *po' branco*, or white powder, being transferred to a perfectly healthy vine, immediately infected it.

In the Alto-Douro the oranges, lemons, citrons and limes have all been blighted, and every kind of vegetable appears to be suffering from sickness.

The vines which suffered most in the Alto-Douro, in 1853, were the Muscatel, Malvazia, Alvarilhão, Ferral, Agadanho and Senzão.

Since my arrival in this country I have noticed that the vines grown on walls in the open air, vines grown in greenhouses, vines grown in hot-houses, vines forced, all show identically the same effects of the *Oïdium* of last year, as exist on the vines in the Alto-Douro.

Taking into consideration all the circumstances above narrated, I have come to the conclusion,—

That the *Oïdium* is the *cause*, and not the *effect* of the disease; that the inclemency of the season in 1853, by checking the circulation of the sap in the vines, produced a predisposition for disease; that if the *Oïdium* continues to appear on the branches of the vines, it is only too probable that it may in a very few years be destroyed; that the *globules* are a sign of health and not of disease, and have no connexion whatever with the fungus called *Oïdium*; and that if the germ of the *Oïdium*, probably still lurking on the old branches, can be destroyed in the open air as effectually as it appears to have been destroyed under glass, then I feel persuaded that all the vines in the Port-wine districts of the Alto-Douro may be saved.

"Supplementary Note on the proposed Remedies for the Eradication of the Vine-Malady." Received June 1, 1854.

1st. I will take the annual production of wines in the Port-wine districts of the Alto-Douro at 80,000 pipes instead of 90,000, and the number of vines to be treated as diseased at 80,000,000.

2nd. The value of freehold land in that district, for the growth of 1000 vines, or one pipe of wine, may be estimated at 50*l*., yielding an interest or rental of 3*l*. per annum.

3rd. The total freehold value of the vineyards in those districts may be estimated at 4,000,000*l*. sterling, giving an annual revenue of 240,000*l*.

4th. In the event of the disease not being checked in its progress, and the grapes being destroyed this year in the Alto-Douro, a *minimum* loss of 240,000*l*, will be sustained, and should the vines perish, the loss may be 4,000,000*l*.

5th. Portugal is said to produce annually 1,000,000 pipes of wine of all sorts and qualities, but I will estimate the total production at 800,000 pipes, and the total number of vines in the country at 800,000,000.

If *Flour of Sulphur* be used, the leaves, branches and shoots are first moistened as equally as possible with a syringe; then the whole is dusted with sulphur, which adheres to the moistened surface.

This operation would have to be repeated thrice, and would consume two ounces of sulphur for every vine, in each of the operations, making a total of 480,000,000 ounces, or about 13,392 tons for the treatment of the 80,000,000 vines in the Alto-Douro, and 133,920 tons for the vines of the whole country.

Sulphur would not cost less than 10*l*. per ton, delivered in the centre of the Alto-Douro districts, or in any other part of the interior of Portugal. The expense of sulphur required for the Douro would be 133,920*l*., and for the whole country 1,339,200*l*.

One man could moisten one vine in one minute, and another man could dust it with sulphur in the same time, so that two men could perform the complete operation on about 700 vines daily, at a cost

of 1*s*. 3*d*. each man for labour, making a total of 14,285*l*. in the Alto-Douro, and 142,850*l*. for all Portugal.

I will suppose that there are 4000 vineyards in the Alto-Douro, planted each with 20,000 vines. The first cost of syringes and fumigators would amount to not less than 10*l*. for each vineyard, or a total of 40,000*l*. for the Alto-Douro.

One quart of water would be required for every vine in each operation, making a total of about 90,000 pipes, the cartage of which, and the labour of distributing it over the mountain vineyards, in tubs, on men's heads, would cost a minimum of 10*s*. per pipe, or a total of 45,000*l*. for the Alto-Douro, and 450,000*l*. for the whole country.

Recapitulation.

	In the Douro.	In the whole country.
For sulphur, say	£135,000	£1,350,000
For labour, at £15,000 for each of the three operations	45,000	450,000
For water, at £45,000 for each of the three operations, or as much as the sulphur.....................	135,000	1,350,000
For instruments	40,000	400,000
	£355,000	£3,550,000

This is independent of any charge for factors or superintendents, or for the extra expense in treating vines and vineyards which are so much further apart than are those in the Alto-Douro.

This expense to be incurred *in the endeavour* to save *one year's crop*, would be equal to a charge of 4*l*. 10*s*. per pipe, or to a *year and a half's rental* of the vineyards, or to more than the whole revenue of Portugal for an entire year.

If a *solution* of *lime* and *sulphur* be employed instead of *flour of sulphur*, the operation would not be less expensive.

If, in *conjunction with the sulphuring* of the branches, the roots were to be exposed, and sulphur and lime thrown upon them, I could not estimate the total expense at less than $1\frac{1}{4}d$. to $1\frac{1}{2}d$. per vine, which would entail a charge equal to another year and a half's

rental of the vineyards, or 18 per cent. on their freehold value for the chance of saving one year's crop.

Again, if the trunks of the vines be bored and the sulphur inserted, this most delicate operation could only be performed by the factors themselves, and if the vines were to be cut down to the ground and grafted with cuttings from sound vines, the entire operation (which could only be performed by the factors) would cost $1\frac{1}{4}d.$ to $1\frac{1}{2}d.$ for each vine, or as much as the sulphuring process; and besides this, there would be a loss of four years' produce at 3*l.* per pipe per annum, making a total loss of 16*l.* 10*s.* in every vineyard growing vines capable of yielding one pipe of wine, or about $33\frac{1}{3}$ per cent., or one-third of the freehold value of the estate.

Lastly, the dressing of the trunk and branches of 800,000,000 vines with mineral tar could not be carried into operation within any reasonable period, on account of the tediousness of the process and the scarcity of labourers. The expense of the tar would also be a bar to its being used.

November 16, 1854.

Colonel SABINE, R.A., V.P. and Treasurer, in the Chair.

J. Lockhart Clarke, Esq., and Captain Moore, R.N., were admitted into the Society.

The following communications were read:—

I. Letter from Lieutenant MAURY to Admiral SMYTH, For. Sec. R.S.

"National Observatory, Washington,
October 21, 1854.

"SIR,—I have the honour to state, for the information of the Royal Society, that a new asteroid was discovered here by Mr. James Ferguson, Assistant Astronomer, at 11 P.M., 2nd of Sept. 1854.

"He was observing Egeria at the time, and found that, the 13th, and this, the 31st, in the field together.

"I have delayed this communication, waiting to ascertain whether the planet might not have been discovered by observers in other parts of the world; and it appearing that it had not, the priority of the discovery, therefore, belongs to the National Observatory; and this new star is added to the family of asteroids as the first representative of America among them, and a memorial of her zeal in the cause of astronomy.

"As a testimony of the high appreciation in which the talents and the industry of Mr. Ferguson are held, the honour of naming this planet was left to him. Following the rule adopted by astronomers with regard to the asteroids, he has selected the graceful name of Euphrosyne.

"Its approximate ephemeris, with the last observations, are herewith enclosed.

"I have the honour to be,

"Respectfully, &c.,

"M. F. MAURY,

"*Lieut. U.S.N.*"

"*Rear-Admiral W. H. Smyth, R.N.*"

Ephemeris of Euphrosyne.

M. T. Washington.		α.	δ.
	h m s	h m s	° ′ ″
1854, Oct. 19.	9 26 41·9	1 12 15·11	−1 56 7·21

Elements of Euphrosyne, computed by Prof. Keith, from observations of Sept. 2nd, 6th and 10th.

M.	13° 36′ 33″·3	Sept. 2·721 M. T. Greenwich.
Π	352 5 50·6	M. Equ. 1854 0.
Ω	33 29 21·7	
i	22 39 13·6	
ϕ	4 22 30·2	
log a	0·469530	
log μ	2·845712	

Ephemeris for October.

M. T. Berlin.	α.	δ.	log r.	log Δ.
	h m s	° ′ ″		
1854, Oct. 19·5	1 12 0	1 59 21	0·43828	0·24622
23·5	1 7 49	1 47 29	0·43850	0·24937
27·5	1 3 49	1 33 49	0·43873	0·25345
31·5	1 0 3	1 18 18	0·43897	0·25861

II. Letter from W. GRAVATT, Esq., F.R.S., to Col. SABINE, Treas. R.S.

The writer announced the arrival in London of the Swedish Calculating Machine constructed by Mr. SCHEUTZ.

III. "Observations on the Respiratory Movements of Insects." By the late WILLIAM FREDERICK BARLOW, F.R.C.S. Arranged and communicated by JAMES PAGET, F.R.S. Received August 20, 1854.

This essay contains the greater part of a series of observations made between 1845 and 1850. The following are some of the conclusions which they plainly indicate:—

(1.) The respiratory movements of Dragon-flies (Libellulæ), and, probably, of other insects also, are naturally subject to considerable and frequent variations in force and rate, the causes of many of these variations being as yet unknown.

(2.) The respirations of these insects are always quickened by exercise, emotion, rise of temperature, galvanism, and mechanical irritation; and the last three agents quicken them in the decapitated, as well as in the perfect, insect.

(3.) The respiratory movements of each segment of the trunk are, in some measure, independent of those of the rest, although in the perfect insect they concur in all the segments. They continue to be performed, though feebly and slowly, in separated segments, provided their nervous cords and ganglia are entire: and they may be abolished in single and successive segments by the local action of chloroform.

(4.) The removal of the head, including the supra- and sub-œsophageal ganglia, does not, like the removal of the medulla oblongata of the vertebrate animal, put a stop to the respiratory movements of the insect; but it diminishes their frequency and force, and deprives them of all influence of the will and of mental emotions.

(5.) The shock inflicted by the sudden destruction of the head, or of the terminal part of the abdomen, generally stops all the respiratory movements of the insect for a time, and much enfeebles them during the remainder of its life.

(6.) The general tendency of the observations is to corroborate the opinion of the self-sufficiency of the several ganglia for the movements of their appropriate segments, and, thus far, to maintain the belief in their essential independence. At the same time, the ob-

servations on the diffused influence of shocks accord with those of the coordinate similar movements of all the segments, in proving their close mutual relations and mutual influence.

November 23, 1854.

THOMAS BELL, Esq., V.P., in the Chair.

W. C. Williamson, Esq., was admitted into the Society, in accordance with the Statutes.

The Chairman gave notice of the ensuing Anniversary Meeting, and read the names of the noblemen and gentlemen proposed as Council and Officers for the ensuing year.

The following gentlemen were elected Auditors of the Treasurer's Accounts on the part of the Society:—

Sir B. Brodie, Bart., C. Darwin, Esq., Dr. Hofmann, James Paget, Esq., and Robert Stephenson, Esq.

The following communications were read:—

I. "On the Impregnation of the Ovum in the Stickleback." By W. H. Ransom, M.D. Communicated by Dr. Sharpey, Sec. R.S. Received October 20, 1854.

I purpose placing before the Royal Society in this communication, the principal results of experiments made during the months of June and July last, on the impregnation of the ovum in *Gasterosteus leiurus* and *G. pungitius*, and hope to be able to furnish a more detailed account of my observations on a future occasion.

The ovarian ovum of these fishes, at a very early stage of its development, is provided with a proper investing membrane, the future chorion. At a later period, one portion of this membrane presents a number of cup-shaped pediculated bodies scattered over its surface, and in the centre of this part of the chorion there is a

funnel-shaped depression, pierced by a canal which leads towards the centre of the egg.

In the nearly ripe ovum, the germinal vesicle occupies an excentric position with respect to the egg as a whole, but imbedded in the centre of a semi-solid accumulation of fine granular matter at that part of the surface which corresponds to the funnel-shaped depression; so that the apex of the funnel, projecting inwards beyond the level of the inner surface of the chorion, makes a depression in the centre of the layer of granular matter, and comes nearly into contact with the germinal vesicle.

For convenience of description, the funnel-shaped depression will now be called *micropyle*, and the layer of granular matter before impregnation, *discus proligerus*.

The germinal vesicle disappears before the ovum leaves the ovary, and no remnant of it or its spots can be seen.

A very delicate membrane invests the yelk within the chorion; this membrane is more distinct after impregnation, or after the action of water upon an unimpregnated egg; it may be isolated, and then exhibits a remarkable degree of elasticity. It is not a *yelk-membrane*, and it will be spoken of as the *inner membrane*.

The layer of the yelk immediately internal to the inner membrane passing over the discus proligerus, is formed by yellowish highly refractive drops which disappear in water, undergoing some remarkable changes, and by a fluid substance which water precipitates in a finely granular form.

The principal mass of the yelk consists of a clear and very consistent albumen. The oil is collected into a few very large drops which come up to the surface.

When the ovum escapes from the ovary, it enters a cavity which may be considered as the ovarian extremity of the oviduct, in which a considerable quantity of clear viscid fluid is previously secreted and collected, to be expelled with the ova.

More exact observation of the micropyle in the free eggs proves that the inner end of the canal is either open, or at most closed by a very delicate membrane. When looking into the funnel from the wide mouth, the apex being in focus, a bright, clear, round or oval spot, such as an aperture would produce, is always visible. If a

section be made of the egg, and the apex brought into focus from within, the same clear spot is well seen, and the fine and regularly dotted structure of the chorion is seen to cease suddenly at the margin of the clear spot.

The general form of the egg after deposition is round, but it is rendered irregular by indentations caused by the pressure of other eggs. It is inelastic, and retains impressions made in it by a needle; and when placed in water, these characters remain for a long time if it be not impregnated,—a fact which indicates that water does not pass through the micropyle, or by imbibition through the chorion. The viscid secretion of the oviduct which invests the eggs may defend them against the action of water, in which it does not readily diffuse or dissolve. This secretion has an alkaline reaction. The substance of the yelk has a decidedly acid reaction,—more than enough to neutralize the alkalinity of the viscid secretion. This reaction is, I believe, due to a peculiar organic acid, but the experiments relating to this question are not yet complete. The seminal particles of the male continue to move for a considerable period in the viscid secretion which envelopes the ripe ova, but they very quickly become still in water.

In the act of impregnation one or more (as many as four have been seen) spermatozoids pass into the micropyle, and probably by their proper motion overcome the obstruction which prevents the entrance of water. Actively moving spermatozoids may remain in contact with the chorion for eighteen minutes at least without producing any sensible change in the ovum, provided none of them enter the micropyle, but when one is seen to enter, in about a quarter of a minute a change is observable.

The changes which are observed to follow the entrance of the spermatozoids into the micropyle are the following:—In about a quarter of a minute the tube is shortened, and very soon a clear space becomes visible within the chorion near the micropyle: this space, or respiratory chamber, gradually extends to the opposite pole of the egg and increases in diameter, as does also the whole ovum. During the formation of this space the surrounding fluid enters through the micropyle, and this gradually retracts and is at length closed. This entrance of fluid into the egg effaces the depressions, restores the

round form, and makes it firm and elastic; but does not cause any such precipitation of granular matter as is produced by its artificial introduction.

While the respiratory chamber is yet in progress of formation, the yellow drops of the superficial layer of the yelk grow pale and disappear; the change beginning near the micropyle. As a result of this, the whole egg becomes clearer, and the *discus proligerus*, which may be now more correctly denominated the *germinal mass*, is more distinct.

The yelk now very slowly alters its form, one surface becoming flattened; but about fifteen or twenty minutes after impregnation a remarkable and more vivid contraction begins, causing the yelk to pass through a series of regularly recurring forms. The contraction begins on one side near the equator, and soon forms a circular constriction which gives the yelk the figure of a dumb-bell, the longer axis of which is the polar axis of the egg. The constriction travels towards the germinal pole, and next produces a flask-shaped figure; this is at length lost by the constriction passing on, and the round form is regained in about a minute. This wave reappears and travels forward again without any distinct period of rest, and I have seen these movements continue for forty-five minutes, though towards the latter part of this period they are less distinct and more limited in extent. The germinal mass has itself during these contractions, which strongly resemble the peristaltic movements of the intestine, undergone changes in form, and has increased in bulk and distinctness. These movements are unaffected by weak galvanic currents.

During the passage forward of each wave of contraction there is an oscillation of the whole mass of the yelk, so that its germinal pole passes once to the right and once to the left of the micropyle, to which it at first corresponded. The plane of this oscillation may be vertical, horizontal, or inclined, but always cuts the micropyle; it begins and ceases with the contractions already mentioned, and would seem to be a mechanical result of them.

For some time before cleavage begins, the only changes of form are the appearance of wave-like elevations and depressions along the under surface of the germinal mass, and its alternate concentration and diffusion. Cleavage begins in about two hours after impreg-

nation; no embryonic cell was observed before it began, nor in any of the cleavage masses.

The inner membrane is folded in during cleavage; it is easily seen thrown into folds at the cleft, and for this reason I do not consider it a *yelk-membrane*, which term would be better applied to the chorion.

II. "On the Applicability of Gelatine Paper as a Medium for Colouring Light." By Horace Dobell, Esq. Communicated by James Paget, F.R.S. Received November 9, 1854.

The object of this communication is threefold.

(1.) To point out the properties of a material called Gelatine Paper, which render it applicable as a medium for colouring light.

(2.) Through the means of gelatine paper, to introduce the use of coloured light in the arts for the preservation of the sight of artisans.

(3.) To introduce the use of gelatine paper for the relief of persons suffering from impaired vision; for the preservation of the sight of travellers, and of all those who are much engaged in reading.

This material was invented in 1829 by the late M. Grenet, of Rouen, and was exhibited by him in its present state of perfection at the Great Exhibition of 1851. But up to the present time it has not been successfully applied to any more useful purposes than the manufacture of artificial flowers, address-cards, tracing-paper, wafers, wrappers for confectionary, and the like.

It is commonly manufactured in sheets, measuring 22 inches in length and 16 inches in diameter, which are sold at a small price; but the sheets can as easily be made of any dimensions not exceeding those of which plate-glass is capable. It can be made of any thickness, from that of the finest tissue paper upwards. It may be obtained as transparent as the best glass, and more free from colour, or of all colours and shades of colour, without interfering with its transparency. It is exceedingly light, and may be bent or rolled up without injury. It can be cut with scissors like ordinary paper,

and may easily be stitched with a needle and thread. By means of an aqueous solution of gelatine, it can be made to adhere accurately to plates of glass without any interference with its transparency. When varnished with collodion it becomes perfectly waterproof, more pliable, capable of bearing a considerable degree of heat without injury, and its transparency is not affected.

Hence it appears, that, in addition to its transparency and susceptibility to various colours and forms, gelatine paper is cheap, portable, and durable.

Such being the properties of the material, the following are enumerated by the author as some of the forms in which he suggests that it may be employed, and in which it has already been found useful.

1. A small sheet of very pale green or blue gelatine paper, to be used in reading. The sheet is simply to be laid upon the page of the book, and the reading to be conducted through the coloured medium. If used in a faint light, the reading paper is to be raised a little from the book to admit more light beneath it.

2. A sheet of gelatine paper of pale green set in a light frame, and placed like a screen before the window or lamp of the engraver, the watchmaker, the jeweller, and the like; thus providing a light of genial colour in which they may pursue their occupations.

3. A similar appliance to the last-mentioned for the use of needlewomen. For this purpose screens are to be provided, both of green and of blue gelatine paper; so that the white materials employed in needlework may be changed to a pleasant green, by the screen of that colour, the yellow materials to a green by the blue screen, and by one or other of these screens the reds softened down into violets or browns.

4. For either of the two last purposes on a larger scale, the gelatine paper may be attached to the window glass of the apartment, thus colouring, if necessary, all the light admitted during daylight.

5. Shades for the eyes in certain affections of the sight, to take the place of the green or blue silk and card shades worn by many persons. The gelatine paper being transparent, will allow the wearer to see his way about, at the same time that the eyes are protected from a glaring light. This may be especially useful in cases

where it is desired not only to shade a diseased eye, but also to protect its nerves from strong light admitted by the sound eye. When not only coloured light but a certain degree of darkness is required, this can be readily and delicately graduated by employing shades of different depths of colour.

6. Masks of gelatine paper for protecting the eyes of travellers against the glare of snow-fields and of sandy deserts.

III. "On the Theory of Definite Integrals." By W. H. L. Russell, Esq., B.A. Communicated by A. Cayley, Esq., F.R.S. Received October 30, 1854.

I propose in the following paper to investigate some new methods for summing various kinds of series, including almost all of the more important which are met with in analysis, by means of definite integrals, and to apply the same to the evaluation of a large number of definite integrals. In a paper which appeared in the Cambridge and Dublin Mathematical Journal for May 1854, I applied certain of these series to the integration of linear differential equations by means of definite integrals. Now Professor Boole has shown, in an admirable Memoir which appeared in the Philosophical Transactions for the year 1844, that the methods which he has invented for the integration of linear differential equations in finite terms, lead to the summation of numerous series of an exactly similar nature, whence it follows that the combination of his methods of summation with mine, leads to the evaluation of a large number of definite integrals, as will be shown in this paper. It is hence evident that the discovery of other modes of summing these series by means of definite integrals must in all cases lead to the evaluation of new groups of definite integrals, as will also be shown in the following pages. I then point out that these investigations are equivalent to finding all the more important definite integrals whose values can be obtained in finite terms by the solution of linear differential equations with variable coefficients. Again, there are certain algebraical equations which can be solved at once by Lagrange's series, and by common algebraical processes; the summation of the former by means of definite

ntegrals affords us a new class of results, which I next consider. A continental mathematician, M. Smaasen, has given, in a recent volume of Crelle's Journal, certain methods of combining series together which give us the means of reducing various multiple integrals to single ones. The series hitherto considered are what have been denominated "factorial series;" but, lastly, I proceed to show that analogous processes extend to series of a very complicated nature and of an entirely different form, and for that purpose sum by means of definite integrals certain series, whose values are obtained in finite terms in the "Exercices des Mathématiques" by means of the Residual Calculus. The total result will be the evaluation of an enormous number of definite integrals on an entirely new type, and the application of definite integrals to the summation of many intricate series.

November 30, 1854.

Anniversary Meeting. A Report of the Proceedings will appear in a future Number.

December 7, 1854.

Colonel SABINE, V.P., in the Chair.

The Chairman announced that the President had appointed the following noblemen and gentlemen Vice-Presidents for the ensuing year:—

The Earl of Rosse, K.P., M.A.
Colonel Sabine, R.A.
Sir Benjamin Brodie, Bart.
Thomas Bell, Esq.
Charles Darwin, Esq., M.A.
Charles Wheatstone, Esq.

Robert Hunt, Esq., was admitted into the Society.

The following communications were read :—

I. "On the Attraction of the Himalaya Mountains, and of the elevated region beyond them, upon the Plumb-line in India." By the Venerable JOHN HENRY PRATT, M.A., Archdeacon of Calcutta. Communicated by the Rev. Professor CHALLIS, M.A., F.R.S. &c. Received October 23, 1854.

The author commences by observing that it is now well known that the attraction of the Himalaya Mountains, and of the elevated region beyond them, has a sensible influence on the plumb-line in North India. This circumstance was brought to light during the progress of the great trigonometrical survey of that country. It was found by triangulation that the difference of latitude between the two extreme stations of the northern division of the arc, namely Kalianpur and Kaliana, is $5° 23' 42''·294$, whereas astronomical observations show a difference of $5° 23' 37''·058$, which is $5''·236$ less than the former.

That the geodetic operations are not in fault, appears from this; that two bases, about 7 miles long, at the extremities of the arc having been measured with the utmost care, and also the length of the northern base having been computed from the measured length of the southern one, through a chain of triangles extending about 370 miles, the difference between the measured and the computed lengths was only 0·6 of a foot, which would produce, even if wholly lying in the meridian, a difference of only $0''·006$ in the latitude.

The difference $5''·236$ must therefore be attributed to some other cause. A very probable cause presents itself in the attraction of the superficial matter which lies in such abundance on the north of the Indian arc. It is easily seen that this disturbing force acts in the right direction, that is, it diminishes the difference of astronomical latitude between the two stations. Whether the cause here assigned will account for the error in the difference of latitude in *quantity* as well as in direction, is the question which the author proposes to discuss in the present paper.

It might seem at first sight that if mountain attraction were so influential as is here supposed, it would disturb the geodetic operations, since in observing the altitude or depression of one station as seen from another, the error in the plumb-line must come into calculation. The author shows, however, by mathematical calculation, that the effect of mountain attraction on the geodetic operations is perfectly insensible, so that it is clearly the astronomical operation of finding the difference of latitude that requires the correction. This is further apparent from the results obtained by Colonel Everest on attempting to determine the azimuths of the arc at seven stations *astronomically*.

To show the importance of considering mountain attraction in the delicate problem of the figure of the earth, the author investigates the effect of a small error in the difference of latitude of the extremities of an arc on the deduced value of the earth's ellipticity. As two unknown quantities occur in the determination of the spheroid of revolution which most nearly agrees with the earth, namely a the equatorial radius and ε the ellipticity, two arcs are required in order to determine them. The author selects the Russian arc, measured near North Cape, as the most advantageous with which to combine the northern portion of the Indian arc, and shows that an error of $5''\cdot236$ in defect in the amplitude of the latter would diminish the value of the ellipticity resulting from the two by about the $\frac{1}{25}$th part of the whole. If the effect of mountain attraction be as great as the author calculates it to be, ($15''\cdot885$ in the northern portion of the Indian arc,) the ellipticity would be diminished by $\frac{1}{8}\varepsilon$, and even by as much as $\frac{1}{6}\varepsilon$ if the whole Indian arc from Kaliana to Damargida were employed.

The author then proceeds, first to develope his method of calculation, and then to reduce his formula to numbers, according to the best data which he was able to collect.

An expression is first investigated for the horizontal attraction of a prism of the earth's crust standing on a given small base, having a small height, and situated at a given angular distance (measured from the centre of the earth) from the station, A, at which the attraction is sought. In the cases to which this expression is employed

it reduces itself without sensible error to

$$\frac{M}{a^2}\cos\left(\frac{1}{2}\theta\right),$$

where M is the attracting mass, a the chord joining its base with A, and θ the angle subtended by this chord at the earth's centre.

In applying this expression to the problem in hand, the author divides the earth's surface into lines, by vertical planes passing through at equal angular distances. These lines are further subdivided by small circles having A for their common pole, and in this manner cutting the whole surface into curvilinear quadrilaterals. He then investigates what the law of dissection must be, that is, according to what law the radii of the small circles must be taken to increase, in order that the horizontal attraction of the portion of the crust standing on one of the quadrilaterals may be equal to the product of its average height and density by a constant quantity, independent of the distance of the quadrilateral from A. If α and $\alpha+\phi$ be the angular radii of two consecutive small circles, there results

$$\frac{\phi\cos^2(\frac{1}{2}\alpha+\frac{1}{4}\phi)}{\sin(\frac{1}{2}\alpha+\frac{1}{4}\phi)}=\text{a constant quantity}=c.$$

To fix the value of this constant, the author assumes $\phi=\frac{1}{10}\alpha$ when ϕ and α are indefinitely small, which gives $c=\frac{4}{21}$. The above equation may then be solved numerically with sufficient approximation. In this manner a table is calculated of the radii of the successive small circles.

These distances should be laid down, and the circles drawn, on a map or globe, as well as the lines dividing the surface into lines. Nothing then remains to be done but to ascertain the average heights of the masses standing on the compartments thus drawn.

The author's paper was accompanied by a plate representing an outline of the continent of Asia. On this was laid down a polygonal figure DEFGHIJKL, (which for convenience the author calls the "enclosed space,") marking the boundary of an irregular mass, which is the only part of the earth's surface that appears to have a sensible effect on the plumb-line in India. The boundary of this space is thus defined:—

DEF is the Himalaya range, having a bend at E from north-west

on the left, to east-by-south on the right. FG is a range running to the table-land of Yu-nan in lat. 25° and long. 103°. GH is the range of the Yun-Ling mountains, in which there are many peaks of perpetual snow. HI is the Inshan range. IJ is the Khing-khan range, very steep on the east side, not so on the west; the passes are said to be 5525 feet above the sea. JK is the Altai range, the highest peak of which is 10,800 feet; the average height is 6000: the range declines towards the east. KL was once thought to be a range of mountains, but is now found to be a line of broken country. LD is the Bolor range, rising to an elevation similar to that of the Hindoo Koosh. There are besides these two ranges of mountains running into the enclosed space, parallel to the Altai and Southern Himalayas, namely the Thian-Schan range, or Celestial Mountains, and the Kuen-Luen range, being a continuation of the Hindoo Koosh, which rises from an altitude of 2558 feet near Herat to about 20,000 where it meets the Bolor range. It is, however, with the elevation of the enclosed space itself that we are principally concerned, since ranges of mountains have not so important an influence, when distant, as table-lands of elevation.

Before describing the country within these limits, the author gives a general sketch of the parts which lie outside, from which it appears that the calculations may be confined to the enclosed space. He then describes in detail the nature of the country within the boundaries of the enclosed space, commencing with the Himalayas, which rise abruptly from the plains of India to 4000 feet and more, and cover an extensive broken space some 100 or 200 miles wide, rising to great heights; perhaps 200 summits exceed 18,000 feet; the highest reaches to more than 28,000. The general base on which these peaks rest rises gradually to 9000 or 10,000 feet, where it abuts on the great plateau north of the range. The character of the country to the south of this plateau is much better known than that to the north. If a circle with a radius 5°·046 (the value of one of the radii employed in the dissection) be drawn around Kaliana, it will pass over the highest part of this plateau. This circle divides the enclosed space into two portions, of which the southern is called by the author the "Known" and the northern the "Doubtful Region." The effects of the two portions are separated in the calculation by the introduction of an arbitrary factor.

After describing the doubtful region, as far as was possible from

the data to which he had access, the author assumes, as the best general representation of the facts, that to the north of a line running through Leh and H'Lassa the doubtful region slopes gradually from 10,000 feet down to 2500 along a parallel line nearly in its centre, and then rises again at the same angle to the north, and that the portion to the south of the line first mentioned, and not included in the known region, slopes at four times that rate.

The author then proceeds to numerical summations replacing an integration to be extended over the whole of the enclosed space. The breadth of the lines employed in the calculation is taken at 30°, which is shown not to be too large to give good results. The following are the results obtained :—

	Arising from		
Station A, Kaliana.	Known region.	Doubtful region.	Total.
Deflexion of plumb-line in meridian	$12''\cdot972$	$14''\cdot881$	$27''\cdot853$
Correction of same for every 100 feet of change in heights	0·312	0·260	
Deflexion of plumb-line in prime vertical	8·136	8·806	16·942
Station B, Kalianpur.			
Deflexion in meridian	3·219	8·749	11·968
Correction for 100 feet	0·059	0·158	
Deflexion in prime vertical	0·789	3·974	4·763
Station C, Damargida.			
Deflexion in meridian	1·336	5·573	6·909
Correction for 100 feet	0·022	0·100	
Deflexion in prime vertical	0·000	2·723	2·723

whence there results,

Total deflexion at A$=32''\cdot601$, and in azimuth $31^\circ\ 18'$ East.
Total deflexion at B$=12\cdot880$, and in azimuth 21 42 East.
Total deflexion at C$=\ 7\cdot426$, and in azimuth 21 31 East.

Difference of meridian deflexions at A and B$=15''\cdot885$.
Difference of meridian deflexions at A and C$=20\cdot944$.
Difference of meridian deflexions at B and C$=\ 5\cdot059$.

The first of these differences is considerably greater than $5''\cdot236$, the quantity brought to light by the Indian Survey.

The author then examines these values more minutely, and considers the effect of various hypotheses for reducing them.

In the first place, the density of the attracting mass may have been assumed too large. The density assumed is 2·75 that of distilled water, the value assumed as the mean density of the mountain Schehallien in the calculations of Maskelyne. This can hardly be too great, but at any rate no remarkable supposition relative to the density can reduce the attraction by more than a small fraction of the whole.

Next, the mass of the doubtful region may have been assumed too great. This hypothesis is then examined by the author, who concludes that even the extravagant supposition of the non-existence of that region will not reduce the difference of meridian deflexions at A and B lower than to $9''\cdot753$.

A third means of reduction may be looked for in the known region. A large part of the attraction belonging to this region arises from the Great Plateau. It would be necessary to cut down this plateau as much as 6000 feet to reduce the deflexions at A and B to $5''\cdot236$, even were the whole mass on the doubtful region non-existent; so that it appears to be quite hopeless, by any admissible hypothesis relative to heights, densities, &c., to reduce the calculated deflexion so as to make it tally with the error brought to light by the survey.

After entering into some elaborate calculations confirmatory of the previous results, the author concludes by calculating the form of the Indian arc, that is, by determining what spheroid of revolution,—the axis of revolution being the earth's axis,—would most nearly coincide with that arc without reference to the rest of the earth, the data employed being the lengths and amplitudes of the northern and southern portions of the arc, and of course their sum, and likewise the latitudes, or at least approximate latitudes, of the middle points of the arcs. By using the amplitudes uncorrected for mountain attraction, the author obtains for the value of the ellipticity deduced from the Indian arc alone $\frac{1}{196\cdot3}$, nearly agreeing with $\frac{1}{191\cdot6}$, which is Col. Everest's result; but by using the amplitudes corrected for mountain attraction according to the author's calculation, the ellipticity is reduced to $\frac{1}{426\cdot2}$. He concludes that the arc is more curved than it would be if it had the mean ellipticity of the earth, and

regards the supposition of a general deviation of the earth's surface in that region from the mean spheroidal form as the most satisfactory mode of accounting for the discrepancy.

II. "On the Value of Steam in the Decomposition of neutral Fatty Bodies." By George Wilson, Esq. Communicated by Warren De la Rue, Esq. Received November 30, 1854.

In the course of a long series of experiments conducted on a large scale, the author has observed that the so-called neutral fatty bodies may be resolved, without danger of injurious decomposition, into glycerine and fatty acids, provided the still is maintained at a uniform high temperature, and that a continuous current of steam is admitted into it.

The temperature required to effect the splitting of the fats into their proximate elements varies with the nature of the body itself, but all hitherto tried may be resolved into glycerine and fatty acid at a temperature of 560° Fahr., many at much below that temperature. At a further period it is the author's intention to lay before the Society a detailed account of his experiments, with the confirmatory analyses, but in the mean time he states that palm oil, cocoa-nut oil, fish oil, animal tallow, Bornean vegetable tallow, "Japan vegetable wax" (more properly tallow), and several others have yielded satisfactory results, the fatty acid and glycerine distilling over together, but no longer in combination, and separating in the receiving vessel.

December 14, 1854.

The LORD WROTTESLEY, President, in the Chair.

Robert Mallet, Esq., was admitted into the Society.

The following communications were read:—

I. "The Physical Theory of Muscular Contraction." By CHARLES BLAND RADCLIFFE, M.D., Licentiate of the Royal College of Physicians, Assistant-Physician to the Westminster Hospital, and Lecturer on Materia Medica. Communicated by CHARLES BROOKE, Esq., F.R.S. Received November 18, 1854.

The theory set forth in this paper is, that muscle is *prevented from contracting* by the several vital and physical agencies which act as stimuli upon muscle,—volition, nervous influence, blood, electricity, light, heat, and the rest,—and that *contraction happens on the cessation of stimulation*, by virtue of the operation of that universal principle of attraction which belongs to muscle in common with all matter, and, so happening, that it is a *physical* phenomenon of the same nature as that contraction which takes place in a bar of metal on the abstraction of heat.

This theory is supported by various arguments, some of which are now stated for the first time. It is argued:—

(*a*.) That *nervous influence* cannot cause muscular contraction, (1) because the degree of innervation, as measured by the supply of nerves, is inversely related to the tendency to contraction; (2) because contraction does not take place so long as the nerve gives evidences of electricity (Du Bois Reymond); (3) because, in some instances at least, contraction does not happen so long as the nerve gives evidences of "irritability"—for contraction is not caused by

heat, by acids and alkalies, and by several other chemical and medicinal substances, until the possibility of provoking contraction by the touch of a needle has been destroyed by the action of the agent—until, that is to say, the "irritability" of the nerve has been destroyed by this action (Eckardt); and (4) because the influence of the nervous centre in causing contraction is to suspend the natural electricity of the nerve and muscle. This last conclusion is evident in the fact, that the signs of electricity, which are absent during tetanus, immediately reappear in the muscle and in the portion of nerve connected with it, when the influence of the nervous centre is cut off and the tetanus resolved by dividing the nerve.

(*b*.) That *blood* cannot cause contraction, (1) because the tendency to contraction is inversely related to the supply of blood; thus, this tendency is greater in the voluntary muscles of fishes and reptiles than of mammals and birds—greater in involuntary than in voluntary muscles—greater in the muscles of any given animal during the state of hybernation than during the period of summer life; and (2) because the state of *rigor mortis* may be relaxed more than once, and the lost "irritability" restored to the muscle by the injection of living blood into the vessels (Brown-Séguard).

(*c*.) That *electricity* cannot cause contraction, (1) because there is a constant current of electricity in a muscle during rest, *but not during contraction* (Du Bois Reymond),—because, that is to say, contraction is absent when muscle is in a state of electrical or *polar action*, and present when this state is absent, so that contraction appears to be antagonized by this state of polar action; and (2) because contraction is never coincident with the passage of a current of artificial electricity; for, not only is it true that a muscle does not contract during the time that a current of artificial electricity is passing through it, but *contraction is invariably relaxed if contraction preexisted* (Eckardt). There is, indeed, momentary contraction at the opening or at the closing of the circuit, but this contraction can be shown to be coincident with neutralization of electrical action, which neutralization is consequent upon the momentary opposition of the natural current of the muscle and the artificial current.

(*d*.) That *mechanical agents* cannot *stimulate* contraction, (1) because the electrical phenomena of muscle are opposed to such an idea; thus muscle affords evidences of electricity during rest, but

not during contraction, and hence the probability is that electricity has been discharged when a muscle contracts on being touched by a needle,—a probability which is supported by the analogy which exists between the structure of muscle and the structure of the electrical organ of the Torpedo, and between the circumstances producing contraction on the one hand and discharge on the other (Owen, Faraday, and others); and (2) because the movements of the stomach, or uterus, or any other viscus are not to be accounted for on the supposition that the contractions are stimulated by the contents of the viscus; thus the food accumulates and the stomach expands until the appetite is satisfied, and contraction does not happen until the preliminary processes of digestion are at an end, and thus also the child grows and the uterus expands, and labour pains do not begin until the growth of the child is completed, and the *stimulus* of that growth suspended.

(*e.*) That *heat and cold* do not *stimulate* contraction, because contraction does not happen until the *natural polar action* of the muscle is suspended,—an event which happens equally under either extreme of temperature,—and thus the muscle would seem to contract because the heat or cold extinguishes that polar action of the muscle which antagonizes contraction.

(*f.*) That *light* cannot cause contraction, (1) because it exercises a directly opposite influence upon the irritable cushions of the sensitive plant; and (2) because it is as easy to agree with Bichât, and suppose that light expands the curtain of the iris, as that it causes contraction in sphincter-fibres surrounding the pupil, which fibres have no existence.

(*g.*) That *chemical and mechanical agencies* do not stimulate contraction, because contraction does not happen until the agent has destroyed that polar action of the muscle which antagonizes contraction (Eckardt).

It is argued, also, that the action of the *will* upon muscle is not necessarily that of a *stimulus*, for the will *may act* by withdrawing something from the muscle as well as by communicating something to the muscle, and, if so, then the previous considerations enhance the probability that it acts by withdrawing something.

In the course of the argument it is further shown that this con-

clusion is borne out by the history of the muscular movements which are manifested in the coats of vessels and in the heart, while at the same time this view is found to give the clue to the physical interpretation of "capillary action," and of rhythm, whether this be in the heart or elsewhere.

It is shown, also, that the same conclusion is borne out by the pathology of tremor, convulsion, and spasm,—of those diseases, that is to say, in which muscular contraction is in excess. Thus, (to mention one argument out of many,) the state of circulation which is invariably associated with tremor, convulsion, and spasm, is one which necessarily implies the diminution of all accustomed stimulation in the muscle, for it is a state which borders closely upon syncope or asphyxia.

And, lastly, it is shown that there is nothing in the phenomenon of muscular contraction which need prevent it from being referred to the operation of that common principle of attraction which belongs to muscle *in common with all matter*, and thus the general conclusion is that another barrier between the organic and inorganic world is broken down, and that muscular contraction is an effect of the universal law of gravitation.

There are, however, sundry grave objections to this theory, and one main object of the paper under consideration is to remove them. Thus, for example, if muscle contracts when nervous influence is withdrawn, how is it that it relaxes when the nerve is divided or otherwise paralysed? and if a muscle contracts for want of blood, how is it that it relaxes in syncope, asphyxia, and death? These objections are grave, but not unsurmountable, as the following hints at explanation will serve to show.

It must be understood, then, that that state of polar action which is present in a muscle during rest and absent during contraction, *is re-established immediately after contraction*; it must also be understood that this state of polar action in the muscle is suspended during ordinary muscular contraction by certain changes which take place in the nervous centre, and that it has *died out* when contraction happens after death, as in *rigor mortis*; and the rest is sufficiently simple.

It is quite in accordance with the theory, then, that a muscle should contract when nervous influence is withdrawn, and that it

should relax after the nerve is divided or otherwise paralysed. At the moment when the continuity of the nerve is broken the muscle contracts, because the influence of the nervous centre is cut off; but this contraction cannot continue, because that state of polar action which antagonizes contraction is immediately re-established in the muscle, and in the portion of nerve connected with it. This relaxation, moreover, must continue, if the paralysed muscle be left to itself, so long as the muscle continues to be the seat of this polar action. And, on the other hand, this contraction must return when this action is suspended, or diminished, or extinguished, as indeed it does; thus the muscle contracts when the polar action is suddenly suspended by galvanism or by the touch of a needle; thus it contracts after the paralysis has continued for some time, and when the failure in the nutrition of the muscle has entailed a corresponding failure in its polar action; and thus it contracts in *rigor mortis*, when all polar action is finally extinguished.

It is also in accordance with theory that tremor, convulsion, and spasm should be caused by want of blood, and that they should cease when the circulation fails, as it fails in syncope, asphyxia, or death. During tremor, convulsion, or spasm, the muscles are insufficiently supplied with nervous influence, because the deficient supply of blood to the nervous centres involves a corresponding deficiency in the degree of innervation; but once let the circulation fail below a certain point, and the whole case is altered. During tremor, convulsion, and spasm, the supply of blood to the nervous centres is insufficient to keep up the normal degree of innervation, *but it is sufficient to prevent the nerves from being paralysed,* and hence the contractions in the muscles, for the nerves being conductors, the failure in the action of the nervous centres is propagated along them to the muscles, and of this failure the contractions are the consequence. But if the circulation fails below a certain point, *the nerves are paralysed for want of blood,* and being paralysed, the failure of innervation in the nervous centres, even though this be now complete, does not entail a corresponding failure in the polar action of the muscle, because the nerves are no longer conductors; and not doing this, the polar action of the muscle, which is much more vigorous than that of the nervous centre and nerve, and far less dependent upon the supply of blood, is immediately re-established,

and being re-established, the muscle relaxes (just as it does in the case where paralysis is caused by division of the nerve), and tremor, convulsion and spasm are at an end. Nor is there any doubt that the nerves are paralysed when the circulation fails to the point which is here supposed. Thus, if the circulation in the hand be depressed by immersion in cold water, the sense of touch and the power of movement are partially or wholly destroyed; or if the principal vessel of a limb be tied, the nerves are similarly paralysed until the collateral circulation be established; and in each case, also, the power of provoking "reflex movements" is diminished or destroyed. In either case the nerves are more or less paralysed for want of blood, and, if so, it surely follows that the nerves must be paralysed, and still more effectually, when the circulation fails as it fails in syncope, asphyxia, or death, and when the movement of the blood is almost or altogether at an end. Hence it is quite intelligible that tremor, convulsion or spasm should be caused by want of blood, as is stated in the argument, and that they should cease in syncope, asphyxia, and death; and thus this objection falls to the ground, and with it all objections of the same kind.

Such is an imperfect sketch of the evidence upon which the physical theory of muscular contraction is founded.

II. "On the Structure of some Limestone Nodules enclosed in Seams of Bituminous Coal, with a Description of some Trigonocarpons contained in them." By J. D. Hooker, M.D., F.R.S., and E. Binney, Esq. Received November 23, 1854.

The authors first describe the occurrence of the limestone nodules, which form a continuous bed in the centre of a thin seam of bituminous coal in the lower part of the Lancashire coal-field. The nodules were of various sizes, some weighing many pounds, and caused the coal to bulge out both above and below them, and they were found to be entirely composed of vegetable tissues converted into carbonate of lime and magnesia. Their formation is supposed by the authors to be due to infiltration of water through the superin-

cumbent shales, which were full of fossil shells supposed to be of marine origin, and the aggregation of the mineral matter round centres of vegetable remains. The chemical constituents of the nodules were found to be carbonates of lime and magnesia, sesquioxide and sulphate of iron, with a little carbonaceous matter.

The probability of these nodules representing an average sample of the vegetable constituents of the surrounding coal is then discussed, and attention is drawn to the very great interest and importance that would attach to them were such a view substantiated, as showing the exact nature of the association of plants which is capable of conversion into bituminous coal.

All the plants contained in the nodules were common in other parts of the coal formation, viz. *Calamodendron*, *Halonia*, *Sigillaria*, *Lepidodendron*, *Stigmaria*, *Trigonocarpon*, *Anabothra*, and others; of these the first-named genus occurred in the greatest abundance and as large fragments of fossil wood. Very many of the specimens were sliced, and being reduced to very thin transparent sections, were examined with the view of determining the botanical character of their contents, and the intimate structure of the masses of more or less homogeneous aspect to which they were reduced by decomposition, previous to or during the operation of calcification. The results were very satisfactory, and seemed to indicate that all traces of vegetable structure may be completely obliterated in the substance of highly bituminized coal, which may nevertheless also contain fragments of wood with their tissues preserved.

An account is then given of the examination of the details of structure of *Trigonocarpon*, and this, as well as the comparison of *Trigonocarpon* with the modern genus *Salisburia*, is illustrated by drawings and analyses.

The authors are still engaged with the study of these nodules, with the view of showing the relationship between *Calamodendron*, *Calamites*, *Sigillaria* and *Anabothra*, and the details are preparing for publication.

December 21, 1854.

The LORD WROTTESLEY, President, in the Chair.

James Allman, M.D., was admitted into the Society.

The following communications were read:—

1. "Remarks on the Anatomy of the *Macgillivrayia pelagica* and *Cheletropis Huxleyi* (Forbes); suggesting the establishment of a new genus of Gasteropoda." By JOHN D. MACDONALD, R.N., Assistant-Surgeon H.M.S. Herald. Communicated by Sir W. BURNETT, K.C.B. &c. Received November 23, 1854.

Having examined the anatomy of the *Macgillivrayia pelagica* and several smaller species of pelagic Gasteropoda, not exhibiting the least similarity in the character of their shells, the author found that they all presented a very close relationship to each other in the type of their respiratory organs, and in other points of structure of less importance.

The gills in every instance seemed to be fixed to the body of the animal immediately behind the head, and did not appear to be appended to the mantle, as in the Pectinibranchiata properly so called. They were also invariably four in number, and arranged in a cruciform manner round a central point. They were free in the rest of their extent, elongated and flattened in form, with a pointed extremity, and fringed with long flowing cilia, set in a frilled border. They were, moreover, furnished with muscular fibres, both transverse and longitudinal, and exhibited great mobility when protruded, but lay side by side in the last whorl of the shell when retracted.

The auditory capsules, each containing a spherical otolithe, were

closely applied to the inner and posterior part of the larger or anterior ganglion of the subœsophageal mass.

There were two tentacula, each bearing at the outer side of its base an eye consisting of a globular lens with optic nerve and retinal expansion. The foot was large and very mobile, but a vesicular float has been observed only in *Macgillivrayia*.

The respiratory siphon was either a simple fold of the mantle forming a temporary tube (*Cheletropis*), or a fold whose borders were united through their whole length, leaving an aperture at the end, as in *Macgillivrayia*.

A lingual ribbon with well-marked rachis and pleuræ occurs in all the species. Very perfect labial plates with closely-set dental points arm the mouth in some instances, and probably in all.

The little animals possessing in common the characters here described, nevertheless fabricate shells so very different as to admit of their division into well-marked genera.

The author conceives that the obvious difference between the pectinibranchiate type of respiratory organs and that observed in the group of Gasteropoda now under consideration, affords sufficient grounds for placing the latter in a distinct order by themselves; and as illustrations of it he proceeds to give some details of the structure of the two species mentioned in the title of the paper, whose shells have been already described by the late Prof. E. Forbes, and figured in Mr. Macgillivray's 'Narrative of the Voyage of H.M.S. Rattlesnake.'

In *Macgillivrayia* the disc of the foot is broad and connected by a narrow attachment to the body just beneath the neck; it carries an operculum behind, and is cleft by a notch in front. A raphe observable in the median line, as well indeed as the whole character of this part of the organ, seems to shadow forth the transformation of the single foot of the Gasteropod into the wing-like expansion of the Pteropod.

After describing the labial plates and lingual strap, the eyes and the branchiæ, the author observes that the tubular siphon protrudes from the shell on the left side and seems to indicate the coexistence of a respiratory chamber with naked branchiæ.

The vesicular float, like that of *Ianthina*, noticed by Mr. Macgillivray, consists of an aggregation of vesicles varying both in number

and size in different cases. It is exceedingly delicate, and could not be found in the specimens first obtained, having probably been destroyed or detached from the foot by the force of the water running through the meshes of the net with which they were captured. Its coexistence with an operculum shows that it is not a modification of the latter.

Of the *Cheletropis Huxleyi*, numerous specimens were found in Bass's Straits and in the South Pacific, between Sydney and Lord Howe's Island.

After giving some details respecting the shell and the foot, the author observes that the latter organ was destitute of float, at least in the specimens he obtained, but was furnished with an operculum, which, probably from its extreme thinness and smallness, had escaped the notice of Professor Forbes. He then points out the peculiarities of the respiratory apparatus.

The portion of the mantle which forms the respiratory siphon, is short, and its opposite edges are merely in apposition, without organic union. The branchiæ are of two kinds, covered and naked. The covered gill is single but of considerable length. It is beautifully pectinated, and fringed with long cilia, and, doubtless, represents the respiratory organ of the pectinibranchiate Gasteropoda. The basis of this part is a long and narrow strip of a tough and fibrous material, folded upon itself into a series of loops invested with a coating of epithelium, and richly ciliated along the free border. The naked gills are four in number, similar both in situation and character to those of *Macgillivrayia*. Each gill is of an oval or elongated form, presenting a thin, frilled and corrugated border, beset with long whip-like cilia. In the central parts muscular fibres are distinctly discernible, some disposed lengthwise and others transversely.

The lingual strap is next described, as well as two file-like triturating plates with which the mouth is furnished.

The two tentacula of each side appear as it were enclosed in one envelope, so as to form a single tactile instrument, which bears a large dark eye on its outer side near the base. To this latter organ the tegumentary covering forms a kind of cornea, beneath which is a spherical lens resting on a mass of black pigment, both being inclosed in a little sac; and the optic nerve, emerging from the sub-

œsophageal ganglion, joins the miniature globe and expands into a retina. The author was unable to trace an opening through the pigment for the passage of light, but thinks it probable that, as in the ocelli of insects, such an aperture exists in the central part. The auditory capsules are situated at some distance behind the eyes, and may be distinctly seen with the microscope when the surrounding parts are carefully removed with fine needles. They are of a rounded or oval form, and each contains a beautifully transparent and highly refracting otolithe, much larger than the lens of the eye.

The paper was accompanied with drawings illustrating the principal points mentioned in the description.

II. A paper was in part read, entitled, "On the Development of Striated Muscular Fibre in Mammalia." By WILLIAM S. SAVORY, M.D., F.R.C.S., Tutor of St. Bartholomew's Hospital Medical College. Communicated by JAMES PAGET, Esq. Received December 9, 1854.

The Society then adjourned over the Christmas recess, to meet again on the 11th of January next.

January 11, 1855.

THOMAS BELL, Esq., V.P., in the Chair.

I. The reading of Mr. SAVORY's paper "On the Development of Muscular Fibre in Mammalia," was resumed and concluded.

The author's observations were made chiefly upon fœtal pigs, but they have been confirmed by repeated examinations of the embryos of many other animals, and of the human fœtus.

If a portion of tissue immediately beneath the surface from the dorsal region of a fœtal pig, from one to two inches in length, be

examined microscopically, there will be seen, besides blood-corpuscles in various stages of development, nucleated cells and free nuclei or cytoblasts scattered through a clear and structureless blastema in great abundance. These cytoblasts vary in shape and size; the smaller ones, which are by far the most numerous, being generally round, and the larger ones more or less oval. Their outline is distinct and well defined, and one or two nucleoli may be seen in their interior as small, bright, highly refracting spots. The rest of their substance is either uniformly nebulous or faintly granular.

The first stage in the development of striated muscular fibre consists in the aggregation and adhesion of the cytoblasts, and their investment by blastema so as to form elongated masses. In these clusters the nuclei have, at first, no regular arrangement. Almost, if not quite as soon as the cytoblasts are thus aggregated, they become invested by the blastema, and this substance at the same time appears to be much condensed, so that many of the nuclei become obscured.

These nuclei, thus aggregated and invested, next assume a much more regular position. They fall into a single row with remarkable uniformity, and the surrounding substance at the same time grows clear and more transparent, and is arranged in the form of two bands bordering the fibre and bounding the extremities of the nuclei, so that now they become distinctly visible. They are oval, and form a single row in the centre of the fibre, closely packed together side by side, their long axes lying transversely, and their extremities bounded on either side by a thin clear pellucid border of apparently homogeneous substance.

It is to be observed how closely the muscular fibres of mammalia at this period of their development resemble their permanent form in many insects.

The fibres next increase in length and the nuclei separate. Small intervals appear between them. The spaces rapidly widen, until at last the nuclei lie at a very considerable distance apart. At the same time the fibre strikingly decreases in diameter; for as the nuclei separate, the lateral bands fall in and ultimately coalesce.

This lengthening of the fibre and consequent separation of the nuclei is due to an increase of material, and not to a stretching of the fibre.

Soon after the nuclei have separated some of them begin to decay.

They increase in size; their outline becomes indistinct; a bright border appears immediately within their margin; their contents become decidedly granular; their outline is broken and interrupted; and presently an irregular cluster of granules is all that remains, and these soon disappear.

It sometimes happens that the nuclei perish while in contact, before the fibre elongates; but the subsequent changes are the same.

The striæ generally first become visible at this period, immediately within the margin of the fibre.

The fibre is subsequently increased in size, and its development is continued by means of the surrounding cytoblasts. These attach themselves to its exterior, and then become invested by a layer of the surrounding blastema. Thus, as it were, nodes are formed at intervals on the surface of the fibre. These invested nuclei are at first readily detached, but they soon become intimately connected and indefinitely blended with the exterior of the fibre. All its characters are soon acquired, the nuclei at the same time gradually sink into its substance, and an ill-defined elevation, which soon disappears, is all that remains.

Lastly, the substance of the fibre becomes contracted and condensed. The diameter of a fibre towards, or at the close of intra-uterine life, is considerably less than at a much earlier period.

At the period of birth muscular fibres vary much in size.

The several stages in the development of muscular fibre, above mentioned, do not succeed each other as a simple consecutive series; on the contrary, two, or more, are generally progressing at the same time. Nor does each commence at the same period in all cases.

II. "On the General Integrals of the Equations of the Internal Equilibrium of an Elastic Solid." By WILLIAM JOHN MACQUORN RANKINE, Civil Engineer, F.R.SS. Lond. & Edinb., &c. Received December 7, 1854.

The *First Section* of this paper is introductory, containing a summary of principles already known respecting the elasticity of solids. Those principles are treated as the consequences of the following

Definition of Elasticity, without introducing any hypothesis as to the molecular structure of bodies.

"*Elasticity is the property which bodies possess of preserving determinate volumes and figures under given pressures and temperatures, and which in a homogeneous body manifests itself equally in every part of appreciable magnitude.*"

The investigations are limited by the following conditions :—

1. The temperature of the elastic body is supposed to be constant and uniform.

2. The variations of the volumes and figures of its particles are supposed to be so small, that the elastic pressures may be considered as sensibly linear functions of those variations.

3. It is assumed, that the only force, besides elastic pressures, acting on the particles of the body, is that of terrestrial gravitation.

All possible small variations of volume and figure of an originally rectangular molecule, when referred to three orthogonal axes, may be resolved into six, viz. three linear dilatations or compressions, and three distortions.

In like manner the elastic pressures exerted on and by such a molecule may be resolved into six, viz. three normal pressures, and three tangential pressures.

Those six pressures are connected with each other and with the attractive force acting on the molecule, by three well-known differential equations of the first order.

They are also connected at every element of the surface of the body, by three well-known linear equations, with the components of the external force acting on that element.

The general problem to be solved is, to find the integrals of the first three equations, subject to conditions fixed by the last three.

The six variations of volume and figure of a rectangular molecule are expressed by six small fractions called "coefficients of displacement."

If the differential of each of these fractions be multiplied by the pressure which directly tends to vary it, the sum of the products is the complete differential of a function called the *Potential Energy of Elastic Forces* for the molecule in question, which is sensibly a homogeneous quadratic function of the six fractions. It has twenty-one terms, and twenty-one constant coefficients, which constitute

the *Coefficients of Elasticity* of the body, for the system of orthogonal axes chosen.

Twenty-one equations express the relations between the systems of coefficients of elasticity in a given body for any two different systems of orthogonal axes.

When a body possesses a system of orthogonal *axes of elasticity*, its coefficients of elasticity, when referred to these axes, are reduced to *nine*.

A body *isotropic* with respect to elasticity has but *three* coefficients of elasticity, which are the same for all sets of orthogonal axes, and are connected with each other by an equation.

If the Potential Energy of Elastic Forces be expressed as a homogeneous quadratic function of the *six elastic pressures*, its coefficients constitute the *coefficients* of *compressibility* and *extensibility*, and of *pliability*. They have relations to the coefficients of elasticity which are consequences of the properties of determinants.

The *Second Section* of the paper relates to the problem of the general integration of the equations of the internal equilibrium of an Elastic Solid, especially when it is *not isotropic*. The method of solution consists of the following eight processes :—

I. The centre of gravity of the body being (in general) taken for the origin of co-ordinates, the forces applied to the surface of the body are subdivided into nine systems of "Reduced External Pressures," which are of such a nature, that for any integration of the external forces as originally expressed over a portion of the surface of the body, may be substituted the sum of three integrations of certain of the reduced external pressures over the three projections of that portion of the surface upon the co-ordinate planes.

By such integrations, extended to the whole of the body, are found the mean values of the nine reduced external pressures, which are connected by simple equations with the mean values, or *constant terms*, of the six internal elastic pressures.

The deviations of the reduced external pressures above and below their mean values, constitute nine systems of *variable parts* of those pressures.

II. The *eighteen* coefficients of the three co-ordinates in the *linear terms* of the six internal elastic pressures are determined by means of eighteen equations; viz. three equations of internal equilibrium

between certain of these coefficients and the force of gravity, and fifteen equations formed by means of the conditions of equilibrium of portions of the body cut off by the co-ordinate planes, and planes parallel to them.

III. The six constant terms, and the eighteen linear terms, of the three dilatations or compressions and the three distortions, are computed from the corresponding terms of the internal pressures by elimination, or by means of the coefficients of extensibility and compressibility, and of pliability. The coefficients of the co-ordinates in those twenty-four terms bear linear relations to the coefficients in the linear and quadratic terms of the three projections of the molecular displacement.

IV. The parts of the nine reduced external pressures corresponding to the constant and linear terms of the internal pressures having been determined for each element of the body's surface and subtracted from the nine actual reduced external pressures, there remain nine *residual* reduced external pressures for each such element, which form three systems, each suitable for development in series of trigonometrical functions of a different pair of independent co-ordinates.

V. The parts of the three projections of the molecular displacement, which correspond to each system of residues of the reduced external pressures, are to be expressed by infinite series in terms of the sines and cosines of linear functions of the proper pair of independent co-ordinates, each order of terms containing (except in some special cases) four kinds of trigonometric functions, multiplied by six exponential functions of the third co-ordinate, whose parameters are the roots of an equation of the sixth order, and by twenty-four arbitrary constants.

From the expressions thus formed are to be computed symbolical expressions for the values of the system of residues or transcendental parts of the reduced external pressures, for each pair of independent co-ordinates, which, by the aid of the equation of the form of the surface of the body, are to be transformed into series containing terms in trigonometric functions of the independent co-ordinates only, multiplied by linear functions of the arbitrary constants, which are (in general) twenty-four times as numerous as the orders of terms.

VI. By equating the constant factor of each term of the symbolical developments thus formed, to the constant factor of the corresponding term of the arithmetical developments found by the process IV., there are formed as many linear equations between the arbitrary constants and known quantities as there are constants to be determined, from which equations those constants are found.

VII. Cases in which one ordinate intersects the surface of the body in two or more pairs of points are to be treated by a special method.

VIII. The results of the previous processes are to be combined, and the solution of the problem completed by determining and adding to them the displacements and rotations of the body as a whole.

The *Third Section* relates to the internal equilibrium of a rectangular prismatic body.

Processes I., II. and III. The determination of the constant and linear terms of the internal pressures, and the corresponding terms of the molecular displacements, consists in the special application of the methods of the preceding section. The axes of figure are taken for axes of co-ordinates.

IV. The means and differences of the transcendental residues of the reduced external pressures on each pair of faces of the prism are developed in series of trigonometric functions of the pairs of independent co-ordinates of the respective faces to which they are applied; the series employed being of such a nature, that for the edges of the body all their terms vanish.

V. and VI. An order having been fixed for the consideration of the forces acting on the three pairs of faces, let yz, zx, xy be that order.

Series of functions trigonometric in y and z, exponential in x, and satisfying the equations of internal equilibrium, with arbitrary constant coefficients, are taken to represent the molecular displacements produced by the residual pressures on the faces normal to x. From those series are computed series representing symbolically those residual pressures, which series being equated to the series numerically representing those pressures, the arbitrary constants are found by elimination.

The formulæ thus obtained are employed to compute ideal systems of external pressures on the faces normal to y and z, called "*provi-*

sional pressures," which are developed in trigonometric functions of the independent co-ordinates of the faces to which they are conceived to be applied. Should the provisional pressures agree with the actual residual pressures on those faces, the process is complete; should they not so agree, the provisional pressures are to be subtracted from the actual residual pressures, leaving systems of remainders called "*secondary pressures.*"

The series representing the molecular displacements corresponding to the *Secondary Pressures* on the faces normal to y are to be found in the manner already referred to. The formulæ thus obtained are to be employed to compute an ideal system of "*Provisional Secondary Pressures*" on the faces normal to z, which are to be developed in trigonometric functions of x and y.

Should the provisional secondary pressures thus found agree with the actual secondary pressures on the faces normal to z, the process is complete. Should they not so agree, the provisional are to be subtracted from the actual secondary pressures, leaving a system of remainders called "*Tertiary Pressures*" on the faces normal to z, whose effects are to be computed in the usual manner.

Process VII. is not required.

Process VIII. consists in combining the terms of the molecular displacements due to the constant and linear terms of the internal pressures, the residual pressures on the faces normal to x, the secondary pressures on the faces normal to y, and the tertiary pressures on the faces normal to z, and finally determining and adding to the other terms, those depending on the displacements and rotations of the prism as a whole.

The *Fourth Section* relates to the integrals of the equations of the internal equilibrium of an isotropic elastic solid.

The constant and linear terms of the internal pressures are to be determined by the methods described in the previous sections, for all solids, whether isotropic or not.

The transcendental terms of the internal pressures and molecular displacements in an isotropic elastic solid, require a special method for their determination.

The three projections of the molecular displacement, with all their functions, in an isotropic solid, are deducible from one primitive function or series of primitive functions of the co-ordinates, by cer-

tain processes of derivation, distributive, but not necessarily commutative.

Each primitive function must satisfy the condition

$$\left(\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}\right)\Phi=0,$$

and may belong to one or other of *eight* classes, according as it is even or odd with respect to x, y and z.

The processes of derivation applicable to the primitive functions contain *three* arbitrary constants for each primitive function. Hence when there is a series of primitive functions of different orders, there are *twenty-four* arbitrary constants for each order of terms.

In the developments of the residual external pressures, there are also *twenty-four* constant coefficients for each order of terms, of which the arbitrary constants are linear functions.

The notation of M. Lamé's work on the Mathematical Theory of the Elasticity of Solid Bodies, so far as it relates to isotropic substances, is compared with that of this paper.

Reference being made to the known method of representing the elastic pressures at a given particle of a solid, in magnitude and direction, by the radii of an ellipsoid, and the positions of the surfaces to which those pressures are applied by those of the tangent planes to an ellipsoid or hyperboloid, the difference (not generally attended to) between the *cone of tangential pressures*, and the *cone of sliding*, is pointed out. This difference is important in the theory of the strength of materials.

January 18, 1855.

Sir BENJAMIN BRODIE, Bart., in the Chair.

The following communications were read:—

I. "Note to a paper read before the Royal Society on the 11th of May, 1854." By J. W. Griffith, M.D., F.L.S. Communicated by Arthur Henfrey, Esq. Received December 13, 1854.

In the paper referred to above, it was stated that the markings or dots upon the valves of the Diatomaceæ, are the optical expressions of depressions existing upon the valves.

All those authors who have paid special attention to the Diatomaceæ, have considered the markings to denote cells; among these we find Ehrenberg*, Kützing†, Ralfs‡, Smith§, and Quekett||.

The evidence I adduced in regard to the more coarsely marked Diatomaceæ, as *Isthmia*, &c., being furnished with depressions and not cells, is, I believe, satisfactory and conclusive; and this view has been admitted in a paper since read before the Royal Society¶.

A different view has been taken of the nature of the *finer* markings, as those upon some species of *Gyrosigma*, by the author of the paper last quoted, as by previous authors; and the object of this note is to direct attention to the support which the extended view argued for by me in the paper above referred to, viz. that the finer markings also correspond to depressions, derives from analogy.

The structure of the Diatomaceæ, and their modes of reproduc-

* Die Infusionsthierchen.

† Die Bacillarien, and Spec. Algarum.

‡ Annals of Nat. History, 1843.

§ British Diatomaceæ.

|| Histological Catalogue of the College of Surgeons; and Lectures delivered before the College of Surgeons.

¶ Proceedings of the Royal Society, June 15, 1854.

tion, are, as is well known, remarkable. So much so, that these organisms have been claimed by botanists as members of the vegetable, and by zoologists as belonging to the animal kingdom. The preponderance of evidence is decidedly in favour of their vegetable nature; but, be this as it may, they must all be classed together,—they form a perfectly natural family. Hence we have a strong argument in favour of the markings upon their valves being identical, and as these are evidently depressions in the genera and species with coarsely marked valves (*Isthmia*, &c.), we should expect from analogy that the same would apply to those with finer markings. And this view receives further support, from the fact, that under varied methods of illumination, corresponding appearances are presented by the markings when viewed by the microscope, from those which are very large, as in *Isthmia*, through those of moderate and small size, as in the species of *Coscinodiscus*, down to those in which they are extremely minute, as in the species of *Gyrosigma*, &c. The angular (triangular or quadrangular) appearance assumed by the markings, arises from the light transmitted through the valves being unequally oblique. This may be readily shown in the more coarsely marked valves (*Isthmia*, *Coscinodiscus*), which present the true structural appearance when the light is reflected by the mirror in its ordinary position, and the spurious angular appearance when the light is rendered oblique by moving the mirror to one side.

II. "Researches on the Theory of Invariants." By WILLIAM SPOTTISWOODE, M.A., F.R.S. Received December 20, 1854.

Invariants may be regarded from two points of view, the permutational and the functional. According to the former they are considered as arising from a process of permutation, according to the latter as derivatives from other functions. In this paper the latter course is adopted; and the following is an outline of the method:—

Let $$u=f(x, y, \ldots a_{\alpha\beta\ldots}, a_{\alpha_{\prime}\beta_{\prime}\ldots}, \ldots)$$

be any homogeneous function of the degree n of the variables $x, y, \ldots$, in which $a_{\alpha\beta\ldots}, a_{\alpha_{\prime}\beta_{\prime}\ldots}, \ldots$ multiplied by their respective mul-

tinomial coefficients are the coefficients of $x^{\alpha} y^{\beta} \ldots, x^{\alpha_{\prime}} y^{\beta_{\prime}} \ldots, \ldots$ Let $x, y, \ldots$ undergo any linear transformation, and let $l, m, \ldots, l', m', \ldots, \ldots$ be the coefficients of transformation; also let

$$\Delta = \begin{vmatrix} l & m & \ldots \\ l' & m' & \ldots \\ \cdot & \cdot & \ldots \end{vmatrix}$$

$$\left.\begin{aligned} \triangleright &= l' \frac{d}{dl} + m' \frac{d}{dm} + \ldots \\ \triangleright' &= l'' \frac{d}{dl} + m'' \frac{d}{dm} + \ldots \\ &\quad \cdot \qquad \cdot \qquad \ldots \end{aligned}\right\}$$

let $\mathrm{a}_{\alpha\beta\ldots}, \mathrm{a}_{\alpha_{\prime}\beta_{\prime}\ldots}, \ldots$ be the new values of $a_{\alpha\beta\ldots}, a_{\alpha_{\prime}\beta_{\prime}\ldots}, \ldots$; also let

$$\nabla = l \frac{d}{dx} + m \frac{d}{dy} + \ldots$$

$$\nabla' = l' \frac{d}{dx} + m' \frac{d}{dy} + \ldots$$

$$\cdot \qquad \cdot \qquad \cdot \qquad \ldots$$

then, à une facteur près,

$$\mathrm{a}_{\alpha\beta\ldots} = \nabla^{\alpha} \nabla'^{\beta} \ldots u$$

and

$$\frac{d\mathrm{a}_{\alpha\beta\ldots}}{da_{\alpha\beta\ldots}}, \quad \frac{d\mathrm{a}_{\alpha\beta\ldots}}{da_{\alpha_{\prime}\beta_{\prime}\ldots}}, \ldots$$

will be respectively equal to the combinations of $l, m, \ldots l', m', \ldots, \ldots$ by which the as are multiplied in $\mathrm{a}_{\alpha\beta\ldots}$. Again, let $f^{\backprime}$ represent a function similar to f, excepting that in the former the multinomial coefficients are dropped; then if

$$\Box_{no\ldots} = f^{\backprime}\left(l, m, \ldots \frac{d}{da_{\alpha\beta\ldots}}, \frac{d}{da_{\alpha_{\prime}\beta_{\prime}\ldots}}, \ldots\right)$$

$$\Box_{\alpha\beta\ldots} = \frac{\triangleright^{n-\alpha} \triangleright^{\beta} \ldots}{1.2\ldots(n-\alpha).1.2\ldots\beta\ldots} \Box_{no\ldots}$$

Now the fundamental condition of invariance is expressed by the following equation:—

$$\Delta^{\frac{mn}{q}} \mathrm{F}(a_{\alpha\beta\ldots}, a_{\alpha_{\prime}\beta_{\prime}\ldots}, \ldots) = \mathrm{F}(\mathrm{a}_{\alpha\beta\ldots}, \mathrm{a}_{\alpha_{\prime}\beta_{\prime}\ldots}, \ldots),$$

$$= \mathrm{F}(a_{\alpha\beta\ldots} \Box_{\alpha\beta\ldots}, a_{\alpha_{\prime}\beta_{\prime}\ldots} \Box_{\alpha_{\prime}\beta_{\prime}\ldots}, \ldots)\mathrm{U},$$

where $\mathrm{U} = \mathrm{F}(a_{\alpha\beta\ldots}, a_{\alpha_{\prime}\beta_{\prime}\ldots}, \ldots)$,

and the problem consists in determining the coefficients of U. This gives

$$\Delta^{\frac{mn}{q}}\ \overline{(\alpha\beta..^{i}},\ \overline{\alpha_{,}\beta_{,}..^{i_{,}}},\ ..)=\sqcap^{i}_{\alpha\beta..}\ \sqcap^{i_{,}}_{\alpha_{,}\beta_{,}..}\ ..U,$$

the quantity within brackets being the coefficient of

$$a^{i}_{\alpha\beta..}\,\mathrm{a}^{i_{,}}_{\alpha_{,}\beta_{,}..}\,..$$

Then if the term be selected in which $i=i_{,}=..=1$, and if $\Delta^{\frac{mn}{q}}$ be so transformed that it shall appear as a function linear in each of the systems

$$\frac{d\mathrm{a}_{\alpha\beta..}}{da_{\alpha\beta..}}=(00),\ \frac{d\mathrm{a}_{\alpha\beta..}}{da_{\alpha_{,}\beta_{,}..}}=(01)..$$

$$\frac{d\mathrm{a}_{\alpha_{,}\beta_{,}..}}{da_{\alpha\beta..}}=(10),\ \frac{d\mathrm{a}_{\alpha_{,}\beta_{,}..}}{da_{\alpha_{,}\beta_{,}..}}=(11)..$$

$$.\qquad\qquad .\qquad\qquad ..$$

and if
$$_{\alpha\beta..}\sqcap=a_{\alpha\beta..}\frac{d}{d(00)}+a_{\alpha_{,}\beta_{,}..}\frac{d}{d(01)}+..$$

$$_{\alpha_{,}\beta_{,}..}\sqcap=a_{\alpha\beta..}\frac{d}{d(10)}+a_{\alpha_{,}\beta_{,}..}\frac{d}{d(11)}+..$$

$$.\qquad .\qquad .\qquad ..$$

then
$$_{\alpha\beta..}\sqcap\,_{\alpha_{,}\beta_{,}..}\sqcap\,..\,\sqcap_{\alpha\beta..}\,\sqcap_{\alpha_{,}\beta_{,}..}\,..=1$$

and U will be at once given by the equation

$$\mathrm{U}=(\overline{\alpha\beta..},\ \overline{\alpha_{,}\beta_{,}..},\ ..)_{\alpha\beta..}\sqcap_{\alpha_{,}\beta_{,}..}\sqcap\,..\Delta^{\frac{mn}{q}}.$$

This method has been applied in the case of two variables to the calculation of quadratic and cubic invariants.

But in the case of two variables the coefficient a may be expressed by a series of symbols with a single suffix, thus:

$$a_0,\ a_{,},\ ..\quad \mathrm{a}_0,\ \mathrm{a}_{,}\ ..,\quad \sqcap_0,\ \sqcap_{,},\ ..\quad {}_0\sqcap,\ {}_{,}\sqcap,\ ..$$

Now since Δ is of the same degree in l, m, and in l', m', the coefficients of all powers and products of a_0, $\mathrm{a}_{,}$, .. in which the degree of l, m is above or below that of l', m', will vanish in the invariant. And from this and some other considerations it is shown, that not only

$$\sqcap^{m}_{0}\mathrm{U}=0,\ \ \triangleright\sqcap^{m}_{0}\mathrm{U}=0,\ ..\ \triangleright^{\frac{mn}{q}-1}\sqcap^{m}_{0}\mathrm{U}=0,$$

but that the coefficients of the invariant may be calculated from the equations arising from equating to zero each term of the last equa-

tion above written, operated upon by the symbol $\triangleright$. This always gives a system of linear equations for finally determining the coefficients. It does not appear possible fully to explain this method without entering into more details than can be given in an abstract.

From the general equations for so determining the coefficients, the number and degrees of the distinct invariants belonging to any given function may theoretically be determined; and this has been done for the simplest case, viz. quadratic functions. But the expressions for higher degrees appear so complicated that an answer to this important question can hardly be expected from this method, in any case not already known.

The view of invariants here taken has suggested a series of other functions of which invariants form the last term. These functions, which I propose to call Variants, may be thus expressed. If functions of the degrees r, s, .. (r, s, .. being less than n) have invariants of the degree m, then writing

$$\partial=\frac{d}{dl},\ \ \partial_{,}=\frac{d}{dm}$$

$$\Delta^{\frac{mr}{2}}\mathrm{F}\left(\partial^{r}\mathrm{a}_0,\ \frac{1}{r}\partial^{r-1}\partial_{,}\mathrm{a}_0,\ ..\ \partial_{,}^{r}\mathrm{a}_0\right)=\mathrm{F}_{m,\,n-r}$$

$$\Delta^{\frac{ms}{2}}\mathrm{F}\left(\partial^{s}\mathrm{a}_0,\ \frac{1}{s}\partial^{s-1}\partial_{,}\mathrm{a}_0,\ ..\ \partial_{,}^{s}\mathrm{a}_0\right)=\mathrm{F}_{m,\,n-s}$$

$$\cdot \qquad \cdot \qquad \qquad \cdot\,\cdot$$

$$\Delta^{\frac{mn}{2}}\mathrm{F}\left(\partial^{n}\mathrm{a}_0,\ \frac{1}{n}\partial^{n-1}\partial_{,}\mathrm{a}_0,\ ..\ \partial_{,}^{n}\mathrm{a}_0\right)=\mathrm{F}_{m,\,0}$$

of which the last is a simple invariant, since, omitting the factor $n(n-1)..2.1$,

$$\partial^{n}\mathrm{a}_0=a_0,\ \frac{1}{n}\partial^{n-1}\partial_{,}\mathrm{a}_0=a_{,},\ ..\ \partial_{,}^{n}\mathrm{a}_0=a_n.$$

With the exact relation between these functions and covariants I am not at present acquainted.

III. A paper was in part read, entitled "Ocular Spectres and Structures as Mutual Exponents." By JAMES JAGO, A.B. Cantab., M.B. Oxon., Physician to the Royal Cornwall Infirmary. Communicated by W. J. HENWOOD, Esq. Received December 26, 1854.

The paper opens by stating that for want of a methodical elimination of ocular spectres from one another—a want which its aim is to meet—physiological optics remain to this day without any real foundation; and even when we have followed the rays of light through all the refracting media of the eye, we cannot safely assert what sensations belong to them until we have detected everything connected with the *percipient membrane* which may obstruct the action of light on it, or which may originate sensations *as of light* through other sorts of impulses. Our eyes in many important respects provide us with an opportunity for microscopical research that no optical instrument employed on the dead eye can rival. We may thus gather a variety of information, physical and physiological, solve points of ocular structure that escape other means of investigation, and bring a profusion of ingenious speculations to a termination, by showing that the phenomena (and this is especially true of the retinal phenomena) which have occasioned them are simply exponential of anatomical facts; and important physiological laws may be arrived at by like means.

The first step in the author's task is to determine the conditions which render objects existing upon or within the eye visible by their shadows, and to obtain optical principles by which we may examine the interior of our own eye with facility, so as to recognize in what lenticular structure, and what part of it, the cause of any shadow or "diffractive image" resides. He shows that we may make every measurement of interest, may decide all the points just alluded to, at the instant, as it were, by mere inspection; and he illustrates his optical principles by appropriate experiments.

The paper then commences its actual elimination of ocular spectres from one another, starting from the appendages of the eye and going on through the ocular tissues in succession to the retina, under several heads, as—

Optical Effects of the Eye-lashes, Eye-lids, and Conjunctival Fluids.

These produce phenomena of reflexion, refraction, and inflection. They may multiply the images of objects which are without or which are within the eye, and occasion us to see the latter. The conjunctival fluids render apt illustrations of "recondite" diffractive shadows.

Optical Structure of the Crystalline Lens.

It is shown that the stelliform figure of our crystalline lens is distinctly visible in divergent light. The lens contains numerous bodies displaying a series of diffractive fringes. The fringes of the border of the iris are likewise conspicuous. Whenever light radiates into the eye from a near point, all these things happen. Hence when a line of radiants (an edge of any body) is before the eye, a mosaic fringe of these coloured shadows will be formed; and there is an *ocular* fringe, as well as the fringe on the edge of a body by light inflected at the body. The ocular colours mentioned seem to have been the cause of the belief that it can be *proved* experimentally that the eye contains no provision for the correction of chromatic dispersion; whereas the colours spoken of should only be compared with those that are produced by *flaws* in the glasses of optical instruments.

The Structure of the Vitreous Body derived from Optical Phenomena.

On this head the author arrives at the following conclusions.

In the vitreous body are innumerable vesicular globules, ranging in size from 0·0008 to 0·005 of a line, which are arranged in unbroken series, in tubes more or less transparent. These tubes precisely resemble veins and arteries in their mode of ramification; they frequently anastomose and are united to one another by capillary plexuses, and they are of less specific gravity than the vitreous fluid. The trunks of this peculiar system of vessels probably arise in the region of, or *at* the base of the optic nerve, and ramify in the vitreous humour; the larger branches passing circumferentially within a limited distance of the hyaloid membrane, and yielding again many branches, which, after repeated subdivisions, end in a capillary network exceedingly subtle and close. Many of the terminal loops of the capillaries are attached to the hyaloid membrane, so as to con-

fine the majority of the branches in a lax manner to its vicinity. A true idea of this system may be gained by conceiving that the veins and arteries here existing in the fœtal eye have in after life been developed according to the growth of the body, but also metamorphosed into these light, peculiar, globule-holding, transparent vessels, and deprived of all foreign support except at their roots and a part of their capillary loops. The intricate ramifications of these vessels have the mechanical effect of in a great degree restraining the relative motion of the humour which fills the hyaloid capsule, and compelling it to concur in the various movements of the eye-ball, so as to obviate the risk of concussion from eddies of the fluid in rapid movements of the eye, and consequent disturbing effects on the lens, the retina and its vessels.

The paper goes on to take this subject up in detail; supplies the dynamical laws which must be kept in view in the application of previously obtained optical methods to the required examination; shows that it is the system of ramifications described which has given rise to the peculiar appearances simulating concentric lamellæ in the vitreous humour previously subjected to chromic acid, so differently interpreted by microscopists. Here too the hitherto vagrant *muscæ volitantes* are, for the first time, invested with form, disposition and office. They are now shown to be the essential element in the structure of the vitreous body; and certain radical misconceptions, as to the nature of these appearances and the constitution of the vitreous body, are pointed out.

The Optical Anatomy of the Retina.

The existence of the *vasa centralia retinæ* in the substance of the retina, and the movements of the blood therein, occasion diversified phenomena. We may examine these vessels in our own eyes, in their minutest distributions, by means of a pin-hole, lens, &c., *in movement across the eye's axis,* in virtue of a physiological law hereafter determined. Currents of blood in these vessels, by pressure upon the nervous matter *at their sides,* produce remarkable phenomena, differing for the superficial and deep vessels (that is, according to the place of the vessel in the five layers of the retina lately discovered by microscopists). These phenomena may all be distinguished from one another, and assigned *with precision* each to its cause.

The phenomena of this kind are *always* before us by daylight and night. In every use of the organs of sight these effects may be observed. In twilight, and *into* night, the pressure of the blood-currents on the retina first equals and then *excels* the impression made by the *failing* external light; and the whole circulatory system may be seen, with proper attention, definitely figuring itself in white or golden colour. A great concentration of light appears at the middle of the retina, which requires a bountiful supply of blood, and owing to the pointing of vessels towards the *foramen centrale*, there is an apparent gyration of *light* currents round a darker pivot. The whole conduct of the retinal circulation may be traced by the *blood-light*. And the manner in which the blood flows through the retina may be equable, or irregular and fitful; it may be very slow, and it may roll with great rapidity. A rhythmical or recurrent circulation of the retinal blood is very frequent, and produces very singular phenomena. We may produce the uncommon states of the retinal circulation at pleasure, by artifices described; and it is shown that it is the retinal circulation which is the cause of all the phenomena which have been taken to prove spontaneous, vibratory, &c. sensations of light.

From these elementary facts being overlooked, fundamental errors as to the conduct of the retina proper have prevailed on all hands. When external light is so faint that the retinal light from blood-pressure exceeds it, *the middle of the retina is so occupied with retinal light* as to be, comparatively with other parts of the retina, unavailable for the usual purposes, and we do not see *anything* with direct nearly so well as with oblique vision; and this inefficiency of the centre of the retina is not limited to the case of "stars of the last degree of faintness" (Herschel and South), but all small objects that are quite visible by "lateral" inspection appear to be "suddenly blotted out" by the eyes being turned directly upon them.

The rhythmical waves of light, or rhythmical progression of the retinal blood (and the mode of movement of the retinal blood as rendered by optical phenomena can be observed by other means), may occur, in a certain sense, spontaneously, or may be produced at will. The retinal circulation may be excited to show astonishing luminous effects.

Among other ways of causing a rhythmical or recurrent movement

of this blood, is that by simple fatigue of the retina by overstraining the sight, when the retina, more or less suddenly (or after a few oscillations), becomes flooded with blood, and complete obliteration of all objects having less than a certain luminosity ensues. This circumstance has misled Brewster and Purkinje, separately, into the belief that they had discovered that a sensation excited in one portion of a retina may be "extended" or "irradiated" to an adjacent portion. Other cases which are imagined by J. Müller and Brewster to support this view are subjected to examination; the real cause of each of the phenomena mentioned being pointed out. Some peculiar effects of retinal light are given; and it is determined that *the rigid correspondence of the limits of sensation with those of the painted image*, is a physiological law literally *absolute*.

Unsuspected difficulties of a solitary eye, and certain well-known phenomena are explained upon the foregoing principles.

May sensation be excited in the trunk of the optic nerve, or centrifugally?

The arguments which have been presumed to prove the affirmative are shown, one by one, to be fallacious, while there is presumption of a negative sort. Observations are offered as to the correct explanation of various physiological points which have been otherwise interpreted, and reputed physiological contrasts of colour are considered.

Images of external objects are painted on the limitary membrane, and perceived by the radial fibres.

This head commences with the quotation of a passage from Sir David Brewster's 'Optics' which he offered towards an explanation of the difficulty of seeing a very faint star by direct vision; and it is shown that the retina is not liable, as Brewster imagines, to be thrown into a state of "undulatory" perception by our looking through the teeth of a "fine comb" or through a single "narrow aperture." The paper points out that the effect observed in these circumstances is produced by our looking near the edge of any body whatever, provided, *and only then*, that the object *move*, be it never so little, *across the eye's axis*. It shows that the same effect is produced by light radiating from a point, by a flame, by lenses, curved reflectors, *whilst they are in the act of moving across* the eye's axis; or by the movement of the eye itself, merely in relation to the light

entering it,—even the naked eye along the sky. The effect produced is shown to be simply owing to this; that the retina, under such action, ceases to perceive in the spaces corresponding to its blood-vessels and capillaries, so that they completely display themselves in the semblance of black bodies (or lines); and the law is arrived at, that the images of external points which are painted on the vessels and capillaries are not perceived when the retina *loses light from one point of space and receives light from another point of space within a certain interval of time*, or that the percipient points lying in front of the vessels *require a certain time to perceive*. A physiological hypothesis is suggested to account for this phenomenon, on the presumption that the "radial fibres," which project from the layer of rods and cones and end in the limitary membrane, are the ultimate percipients of light.

It is pointed out how wonderfully close we may find the correspondence between the microscopical and optical anatomy of the retina. Each pair of identical fibres of the two optic nerves must be regarded as one nerve. Another supposed anomaly to the simplicity of nervous action being explained on anatomical principles, a statement of ordinary optical nervous action is made, and a summary evinces how the anomalies in visual experience are due to the complex additions to a simple organ of sight.

January 25, 1855.

The Right Hon. LORD WROTTESLEY, President, in the Chair.

The Right Hon. The Earl of Harrowby was admitted into the Society.

THE BAKERIAN LECTURE was then delivered by JOHN TYNDALL, Ph.D., F.R.S.; being an oral exposition, illustrated by experiments, of the substance of a paper by him, entitled "On the Nature of the Force by which Bodies are repelled from the Poles of a Magnet; preceded by an Account of some Experiments on Molecular Influences."

The paper commences with an introduction, in which the present aspect of the portions of science to which it refers is briefly sketched. A section is devoted to the examination of the magnetic properties of wood, which substance, the author finds, except where extraneous impurities are present, to be always diamagnetic, and to set in the magnetic field with its fibre equatorial. The influence of the shape of pointed and flat poles is studied, and those curious phenomena of rotation, first observed by M. Plücker, and attributed by him to the action of two conflicting forces, are referred to molecular structure as a cause. Between flat poles, it is proved that the line joining the centres of the two poles is the line of *minimum* force; that is to say, the force increases more quickly from the central point of the magnetic field in an equatorial direction than in an axial one. Reflecting on the great diversity of opinion at present existing with regard to the real nature of the diamagnetic force, the author deemed it necessary to commence at the foundation of the inquiry. A fundamental question in the present case is the following:—Are dia-

magnetic bodies repelled by a magnet in virtue of any constant property possessed by the mass? Is the force in question a mere repulsion of ordinary matter, or is the repulsion exercised in virtue of a state of magnetization into which the body is first thrown? This question is answered in a manner which admits of no doubt.

It is proved that the repulsion of diamagnetic bodies increases in a quicker ratio than the strength of the magnet which produces the repulsion. Within wide limits, indeed, the repulsion, instead of being simply proportional to the strength of the magnet, is proportional to the square of the strength, which leads inevitably to the conclusion that the body thus repelled contributes to the effect produced; that its repulsion is due to an excited condition into which it is thrown by the influencing magnet, the intensity of this excitement varying within the limits already referred to, as the strength of the magnet which produces it. This conclusion is further arrived at by a close comparison of the repulsions of diamagnetic bodies with the attraction of paramagnetic ones: both are found subservient to one and the same law.

It is next proved that the diamagnetic excitement produced by one pole of a magnet is not the state which enables a pole of an opposite quality to repel the substance:—that each pole induces a condition peculiar to itself, or, in other words, that the excitement of diamagnetic bodies in the magnetic field is of a *dual* character.

These points being established, a searching comparison is instituted between the phenomena exhibited by paramagnetic and diamagnetic bodies in three distinct cases:—first, when operated on by the magnet alone; secondly, when operated on by the current alone; and, thirdly, when operated on by the magnet and current combined. A bar of iron was, in some of these cases, compared with a bar of bismuth, but it was soon found necessary, in order to avoid the proved errors of reasoning, to take strict account of the molecular structure of the bismuth. A bar of this substance, cut in a certain manner from the crystallized mass, exhibits between the poles of a magnet precisely the same visible deportment as a bar of iron, while it is well known that the normal deportment of bismuth is opposed to that of iron. The author, in his examination of the points before us, divided paramagnetic bars into two distinct classes, and classified diamagnetic bars in the same manner; one class he

called *normal*, and the other class *abnormal*. A normal paramagnetic bar is one which sets its length from pole to pole in the magnetic field, and a normal diamagnetic bar is one which sets its length at right angles to the line joining the poles. An abnormal magnetic bar, on the contrary, is one which sets its length equatorially; while an abnormal diamagnetic bar is one which sets its length axially.

In all cases, whether operated on by the magnet alone, the current alone, or the magnet and current combined, the deportment of the normal paramagnetic bar is precisely antithetical to that of the normal diamagnetic one. In the magnetic field the former sets axially, the latter equatorially. Operated on by a voltaic current, the former sets its length at right angles to the current, the latter sets its length parallel to it. When magnet and current act together on the bars, it is found that the disposition of forces which produces a deflection from right to left of the paramagnetic bar produces a deflection from left to right of the diamagnetic bar. If the position of equilibrium of the former be from N.E. to S.W., the position of equilibrium of the latter is from N.W. to S.E. In short, the position of rest for the normal magnetic bar is always at right angles to the position of the diamagnetic bar. A precisely similar antithesis is observed when we compare the abnormal paramagnetic bar with the abnormal diamagnetic one. The former, in the magnetic field, sets equatorially, the latter axially. The former sets parallel to an electric current, the latter perpendicular to the same. If the deflection of the former be from right to left, the deflection of the latter, under like conditions, is from left to right. Finally, the position of equilibrium of the former is always at right angles to that of the latter.

But if the deportment of the normal paramagnetic bar be compared with that of the abnormal diamagnetic one, it will be found that they are in all cases identical; and the same identity of deportment is exhibited when the abnormal paramagnetic bar is compared with the normal diamagnetic one. The necessity of paying attention to structure in experiments of this nature could not, it is imagined, be more strikingly exhibited.

It is proved by these experiments that the simple substitution of an attractive force for a repulsive one would completely convert the phenomena exhibited by paramagnetic bodies into those exhibited by diamagnetic ones. That if that which Gauss has called the ideal

distribution of magnetism in an iron bar be reversed, we have a distribution which would produce the phenomena of a bismuth bar of the same dimensions. All the phenomena of diamagnetic bodies become equally intelligible with those of paramagnetic ones, when we assume that the former class possess a polarity the same in kind, but the opposite in direction to that of the latter.

It is well known that a bar of iron surrounded by a helix in which a voltaic current circulates is converted into a magnet, and exhibits that *twoness* of action,—those phenomena of attraction and repulsion at its two ends—to which we give the name of polarity. The present paper contains an account of experiments made with the view of ascertaining whether similar phenomena were exhibited by a bar of pure bismuth. A cylinder of the latter substance, $6\frac{1}{2}$ inches long and 0·4 of an inch in diameter, was suspended by a suitable device within a helix of covered copper wire, so that it could vibrate freely from side to side. The ends of two electro-magnetic cores were brought to bear upon the two ends of the bismuth bar, and it was so arranged that the two magnetic poles acting upon the bar might be of the same or of opposite qualities. The helix being first excited by a strong current, a current of considerably less power was sent round the electro-magnetic cores, and their action upon the bismuth bar was observed: by means of a gyrotrope the current in the helix was reversed, and the effect of the reversion noted; permitting the current through the helix to flow in its last direction, by means of a second gyrotrope, the current which excited the cores was reversed; and finally, while the last magnetic polarity remained unaltered, the direction of the current in the helix was once more changed, and the consequent deportment of the bismuth bar was noted.

In making these experiments and exercising some judgment in the choice of the relation between the strength of the magnets and the strength of the current in the helix, the most complete mastery was attained over the motions of the bar. With one disposition of the forces the ends of the bar of bismuth were promptly repelled, with another arrangement they were just as promptly *attracted*. If, when moving towards the cores, the direction of the current in the helix was reversed, the motion of the bar was at once arrested, and attraction was converted into repulsion. If, while receding from the bar,

the direction of the current in the helix was changed, the recession was stopped and the ends of the bar were attracted. The same results were obtained when, instead of changing the direction of the current in the helix, the polarity of the electro-magnetic cores was reversed. When the latter were so excited that poles of the same quality were presented to the ends of the bismuth bar, the repulsion of the one pole was balanced by the attraction of the other, and under the influence of these opposing forces the bar stood still.

Pursuing the argument further, a south pole and a north pole were caused to act simultaneously upon *each* end of the bismuth bar; supposing one end of the latter to be repelled by a south pole, then, on the assumption of diamagnetic polarity, the same end would be attracted by a north pole; and permitting both poles to act upon it simultaneously from *opposite sides*, we may anticipate that the force tending to turn the bar will be greater than if only a single pole were used. To test this conclusion, *four* electro-magnetic cores were made use of; the two poles to the right of the bismuth bar were of the same name, while the two to the left of the bismuth bar were of an opposite quality: with this arrangement the mechanical action upon the bar was greatly augmented, and the foregoing anticipation completely verified.

The bar employed in these experiments is unusually large, but it does not mark the practical limit of success. All the results obtained with this bar were obtained with another solid cylinder of bismuth 14 inches long and 1 inch in diameter. The corresponding experiments were made with bars of iron, and it was always found that the arrangement of forces which caused the attraction of the ends of the paramagnetic bar caused the repulsion of the ends of the diamagnetic one; while the disposition which caused the repulsion of the ends of the paramagnetic bar produced, in the most manifest manner, the *attraction* of the ends of the diamagnetic one. As regards the abstract question of polarity, the only difference between iron and bismuth is the comparatively intense action of the former. In the case of a magnetic body, whose capacity for magnetization does not exceed that of bismuth, and in which coercive force is equally absent, no proof can be adduced in favour of the polarity of the former body that cannot be matched by proofs of equal value in every respect of the polarity of the latter.

The objections that have been and possibly may be used against diamagnetic polarity, are next considered, and some observations are made on the constitution of the magnetic field. The relation of our present knowledge of diamagnetic action to the theory of Weber, and to Ampère's hypothesis of molecular currents is stated; and in conclusion, the author dwells briefly upon those points of diamagnetic action wherein the views of M. Matteucci differ from his own.

The following paper was read:—

"On Differential Transformation and the Reversion of Serieses." By J. J. SYLVESTER, Esq., F.R.S. Received January 25, 1855.

With a view to its publication in the Proceedings of the Society, I take occasion to communicate the result of my investigations, as far as they have yet extended, into the general theory of differential transformations, containing a complete and general solution of the important problem of expanding a given partial differential coefficient of a function in respect of one system of independent variables in terms of the partial differential coefficients thereof, in respect to a second system of independent variables, each respectively given as explicit functions of the first set.

This question may be shown to be exactly coincident with that of the reversion of simultaneous serieses proposed by Jacobi, which may be thus stated: given $(n+1)$ quantities, each expressed by rational infinite serieses as functions of n others; required to express any one of the first set in a rational infinite series in terms of the other n of the same set. This question has only been resolved by Jacobi for a particular case; the result hereunder given for the transformation of differential coefficients contains the solution of the general question. My method of investigation is entirely different from that adopted by the great Jacobi, and I hope in a short time to be able to lay it in a complete form before the Society, and probably to add a solution of the still more general question comprising the reversion of serieses as a particular case, viz. the question of express-

ing any one of n quantities connected by m equations in terms of any $(n-m)$ others of the same.

Let there be any number of variables, say u, v, w, of which x, y, z, ϑ are given functions, it is required to expand

$$\left(\frac{d}{dx}\right)^f \left(\frac{d}{dy}\right)^g \left(\frac{d}{dz}\right)^h \vartheta$$

in terms of the partial differential coefficients of ϑ, x, y, z in respect of u, v, w.

Form the determinant

$$\begin{matrix} \frac{dx}{du}, & \frac{dx}{dv}, & \frac{dx}{dw}, \\ \frac{dy}{du}, & \frac{dy}{dv}, & \frac{dy}{dw}, \\ \frac{dz}{du}, & \frac{dz}{dv}, & \frac{dz}{dw}, \end{matrix}$$

which call J.

The required expansion will contain in each term an integer numerical coefficient, a power of $\frac{1}{J}$, one factor of the form

$$\left(\frac{d}{du}\right)^p \left(\frac{d}{dv}\right)^q \left(\frac{d}{dw}\right)^r \vartheta,$$

and other factors of the form

$$\left(\frac{d}{du}\right)^l \left(\frac{d}{dv}\right)^m \left(\frac{d}{dw}\right)^p x$$

$$\left(\frac{d}{du}\right)^{l'} \left(\frac{d}{dv}\right)^{m'} \left(\frac{d}{dw}\right)^{p'} y$$

$$\left(\frac{d}{du}\right)^{l''} \left(\frac{d}{dv}\right)^{m''} \left(\frac{d}{dw}\right)^{p''} z.$$

Let the latter class of factors be distinguished into two sets, those where $l+m+n=1$,

$$\begin{pmatrix} li.i\; l=0 & m=0 & n=0 \\ \text{or } l=0 & m=1 & n=0 \\ \text{or } l=0 & m=0 & n=1 \end{pmatrix}$$

which I shall call uni-differential factors, and those in which $l+m+n > 1$, which I shall call pluri-differential factors.

First, then, as to the form of the general term abstracting from

the numerical coefficient and the uni-differential factors (except of course so far as they enter into J). This will be as follows :—

$$\left(\frac{d}{du}\right)^{{}^1l_1}\left(\frac{d}{dv}\right)^{{}^1m_1}\left(\frac{d}{dw}\right)^{{}^1n_1}x\times\left(\frac{d}{du}\right)^{{}^2l_1}\left(\frac{d}{dv}\right)^{{}^2m_1}\left(\frac{d}{dw}\right)^{{}^2n_1}x\times\ldots\left(\frac{d}{du}\right)^{{}^{e_1}l_1}\left(\frac{d}{dv}\right)^{{}^{e_1}m_1}\left(\frac{d}{dw}\right)^{{}^{e_1}n_1}x$$

$$\times\left(\frac{d}{du}\right)^{{}^1l_2}\left(\frac{d}{dv}\right)^{{}^1m_2}\left(\frac{d}{dw}\right)^{{}^1n_2}y\times\ldots\ldots\times\left(\frac{d}{du}\right)^{{}^{e_2}l_2}\left(\frac{d}{dv}\right)^{{}^{e_2}m_2}\left(\frac{d}{dw}\right)^{{}^{e_2}n_2}y$$

$$\times\left(\frac{d}{du}\right)^{{}^1l_3}\left(\frac{d}{dv}\right)^{{}^1m_3}\left(\frac{d}{dw}\right)^{{}^1n_3}z\times\ldots\ldots\times\left(\frac{d}{du}\right)^{{}^{e_3}l_3}\left(\frac{d}{dv}\right)^{{}^{e_3}m_3}\left(\frac{d}{dw}\right)^{{}^{e_3}n_3}z$$

$$\times\left(\frac{d}{du}\right)^{p}\left(\frac{d}{dv}\right)^{q}\left(\frac{d}{dw}\right)^{r}\vartheta\times\frac{1}{J^{\omega}},$$

subject to the limitations about to be expressed.

Call
$$\begin{aligned}{}^1l_1+{}^2l_1+\ldots+{}^{e_1}l &= L_1\\ {}^1l_2+{}^2l_2+\ldots+{}^{e_2}l_2 &= L_2\\ {}^1l_3+{}^2l_3+\ldots+{}^{e_3}l_3 &= L_3,\end{aligned}$$

and form the analogous quantities M_1, M_2, M_3; N_1, N_2, N_3. Then we must have

$$L_1+L_2+L_3+M_1+M_2+M_3+N_1+N_2+N_3+p+q+r$$
$$=f+g+h+e_1+e_2+e_3;$$

and as the sum of any group of indices l, m, n must be not less than 2, we have

$$f+g+h+e_1+e_2+e_3+p+q+r, \text{ not less than } 2e_1+2e_2+2e_3,$$

so that $e_1+e_2+e_3$ must not exceed $f+g+h+p+q+r$; furthermore, $p+q+r$ must not exceed $f+g+h$; and finally,

$$\omega=f+g+h+e_1+e_2+e_3.$$

1. We may first take $e_1+e_2+e_3=E$, giving to E in succession all integer values from $f+g+h$ to $2f+2g+2h$, and find all possible solutions of this equation with permutations between the values of e_1, e_2, e_3.

2. We may then take $p+q+r=s$, giving s in succession all integer values from 1 to $f+g+h$, and find all possible solutions of this equation with permutations between f, g, h.

3. We may then take $L+M+N=f+g+h+E-s$, and find all the values of L, M, N, with permutations allowable between the values of L, M, N.

4. We may then take

$$L_1+L_2+L_3=L$$
$$M_1+M_2+M_3=M$$
$$N_1+N_2+N_3=N,$$

and solve these several equations in every way possible, with permutations as before.

5. We must take

$$\begin{array}{lll} {}^1l_1+{}^2l_1+\ldots+{}^{e_1}l_1=L_1 & {}^1m_1+{}^2m_1+\ldots+{}^{e_1}m_1=M_1 & {}^1n_1+{}^2n_1+\ldots+{}^{e_1}n_1=N_1 \\ {}^1l_2+{}^2l_2 \;\ldots\; {}^{e_2}l_2=L_2 & {}^1m_2+{}^2m_2 \;\ldots\; {}^{e_2}m_2=M_2 & {}^1n_2+{}^2n_2 \qquad {}^{e_2}n_2=N_2 \\ {}^1l_3+{}^2l_3 \;\ldots\; {}^{e_3}l_3=L_3 & {}^1m_3+{}^2m_3 \;\ldots\; {}^{e_3}m_3=M_3 & {}^1n_3+{}^2n_3 \qquad {}^{e_3}n_3=N_3 \end{array}$$

and solve in every possible manner these equations, but without admitting permutations between the values of ${}^1l_1\ {}^2l_1\ldots{}^{e_1}l_1$, or between the values of the members of the other of the third sets taken each *per se*, and subject to the condition that every such sum as ${}^rl_i+{}^rm_i+{}^rn_i$ must be greater than unity. Every possible system of values of these nine sets will furnish a corresponding pluri-differential part to the general term.

Next, as to the uni-differential part, we may form the quantity

$$\left(\frac{dy}{dv}\cdot\frac{dz}{dw}-\frac{dy}{dw}\,\frac{dz}{dv}\right)^{\lambda_1}\left(\frac{dy}{dw}\cdot\frac{dz}{du}-\frac{dy}{du}\,\frac{dz}{dw}\right)^{\mu_1}\left(\frac{dy}{du}\,\frac{dz}{dv}-\frac{dy}{dv}\,\frac{dz}{dw}\right)^{\nu_1}$$

$$\left(\frac{dz}{dv}\,\frac{dx}{dw}-\frac{dz}{dw}\,\frac{dx}{dv}\right)^{\lambda_2}\left(\frac{dz}{dw}\,\frac{dx}{du}-\frac{dz}{du}\,\frac{dx}{dw}\right)^{\mu_2}\left(\frac{dz}{du}\,\frac{dx}{dv}-\frac{dz}{dv}\,\frac{dx}{dw}\right)^{\nu_2}$$

$$\left(\frac{dx}{dv}\,\frac{dy}{dw}-\frac{dx}{dw}\,\frac{dy}{dv}\right)^{\lambda_3}\left(\frac{dx}{dw}\,\frac{dy}{du}-\frac{dx}{du}\,\frac{dy}{dw}\right)^{\mu_3}\left(\frac{dx}{du}\,\frac{dy}{dv}-\frac{dx}{dv}\,\frac{dy}{dw}\right)^{\nu_3},$$

where

$$\lambda_1+\lambda_2+\lambda_3=L+p$$
$$\mu_1+\mu_2+\mu_3=M+q$$
$$\nu_1+\nu_2+\nu_3=N+r.$$

These equations are to be solved in every possible manner with permutations between the members of the λ set, the μ set, and the ν set. Finally, we have to consider the numerical coefficient. To give a perfect representation of this, we must ascertain what identities exist in the factors of the pluri-differential part. Let us suppose that one set of operators upon x is repeated θ_1 times, another

θ_2 times, and so on, giving rise to the powers $\theta_1, \theta_2, \ldots\ldots \theta_\alpha$ in the x line. Similarly, form $\phi_1, \phi_2, \ldots \phi_\beta$ from the y line, and $\psi_1, \psi_2 \ldots \psi_\gamma$ from the z line. Then the numerical part of the general term will be

$$\frac{\Pi(\lambda_1+\mu_1+\nu_1)\Pi(\lambda_2+\mu_2+\nu_2)\Pi(\lambda_3+\mu_3+\nu_3)}{\Pi\lambda_1\Pi\mu_1\Pi\nu_1\Pi\lambda_2\Pi\mu_2\Pi\nu_2\Pi\lambda_3\Pi\mu_3\Pi\nu_3}$$

$$\times\frac{\Pi(L+p)\Pi(M+q)\Pi(N+r)}{\begin{Bmatrix}\Pi^1 l_1\Pi^1 m_1\Pi^1 n_1\Pi^2 l_1\Pi^2 m_1\Pi^2 n_1 \ldots\ldots \\ \Pi^1 l_2\Pi^1 m_2\Pi^1 n_2\Pi^2 l_2\Pi^2 m_2\Pi^2 n_2 \ldots\ldots \\ \Pi^1 l_3\Pi^1 m_3\Pi^1 n_3\Pi^2 l_3\Pi^2 m_3\Pi^2 n_3 \ldots\ldots\end{Bmatrix}}$$

$$\times\frac{D}{\Pi\theta_1\Pi\theta_2\ldots\Pi\theta_\alpha\Pi\phi_1\Pi\phi_2\ldots\Pi\phi_\beta\Pi\psi_1\Pi\psi_2\ldots\Pi\psi_\gamma},$$

where in general Πm means $1.2.3\ldots m$: as regards D, it is the following determinant, viz.

$$\begin{matrix}
\lambda_1+\mu_1+\nu_1 & \nu & \nu & L_3 & M_3 & N_3 \\
\nu & \lambda_2+\mu_2+\nu_2 & \nu & L_2 & M_2 & N_2 \\
\nu & \nu & \lambda_3+\mu_3+\nu_3 & L_1 & M_1 & N_1 \\
\lambda_1 & \lambda_2 & \lambda_3 & L_1+L_2\ L_3+p & \nu & \nu \\
\mu_1 & \mu_2 & \mu_3 & \nu & M_1+M_2+M_3 & \\
\nu_1 & \nu_2 & \nu_3 & \nu & \nu & N_1+N_2+N_3+r
\end{matrix}$$

The result, for greater brevity, has been set out in the above pages for the case of ϑ, a function of three variables, but the reader can have no difficulty in extending the statement to any number. In the case of a single variable, the formula can easily be identified with that given by Burman's law. It is noticeable that the determinant above written is of the form

$$Apqr+Bpq+Cqr+Dqrp+Epe+Fq+Gr,$$

the part independent of p, q, r being easily seen to vanish. Moreover, A, B, C, D, E, F, G, H are all essentially positive, so that D can only vanish (except for $p=0$, $r=0$, $q=0$) by virtue of one condition at least more than the number of the variables.

February 1, 1855.

Colonel SABINE, Treas. and V.P., in the Chair.

The following communications were read :—

I. The reading of Dr. JAGO's paper, "On Ocular Spectres and Structures as Mutual Exponents," was resumed and concluded*.

II. "Micro-chemical Researches on the Digestion of Starch and Amylaceous Foods." By PHILIP BURNARD AYRES, M.D. Lond. Communicated by JOHN BISHOP, Esq., F.R.S. Received January 11, 1855.

After some general historical remarks on the methods hitherto employed in the investigation of the complicated phenomena of the process of digestion, the comparatively small results obtained by chemical analysis of the contents of the stomach, intestinal canal, and of the evacuations, by Tiedemann and Gmelin, Berzelius, and others, the author proceeded to demonstrate the necessity of a minute examination of the contents of the alimentary canal by the microscope, and such chemical tests as we possess for the determination of the changes of such articles of food as exhibit definite structure.

In order that we may ultimately arrive at a complete exposition of the phenomena of digestion, he is of opinion that it will be necessary to examine,—first, the structure of particular kinds of food, then the changes produced in them by cooking, and lastly to trace the changes they undergo at short intervals, through the alimentary canal from the stomach to the rectum. The results of a series of researches of this character on the changes in starch, and starch-containing foods, are presented in this memoir.

* An abstract is given at p. 208.

The method adopted for the examination of the changes in starch and starch-foods was as follows:—An animal was kept fasting twenty-four hours, and afterwards confined to a diet consisting of the starch or amylaceous food, with water, for five or six days, until the debris of all other kinds of food previously taken were cleared from the alimentary canal. At a determinate time, after a meal, the animal was killed, the abdomen laid open as quickly as possible, and ligatures placed at short intervals on the intestinal canal, from the pylorus to the rectum. The contents of the stomach and each portion of the intestinal canal included between the ligatures was then carefully examined. This mode of examination sufficed to determine the changes which occur in the food during normal digestion; but other questions as to the particular secretion or secretions by which the changes observed were effected.

The fluids poured into the alimentary canal are five in number,—the saliva, gastric juice, bile, pancreatic juice, and finally, the intestinal mucus.

The influence of the saliva is easily determined, by chewing the particular food subjected to experiment, and keeping the mixture at about 98° Fahr. The combined action of the saliva and gastric juice is seen in the contents of the stomach. To determine the action of the bile, the common bile-duct was tied, and to ascertain the action of the intestinal mucus, it was necessary to ligature the bile and pancreatic ducts. If the digestion of the substance is not effected in the stomach, it is evident that it cannot be attributed to the saliva or gastric juice; if the digestion is still effected in the intestinal canal after ligature of the bile-duct, it cannot be attributed to the action of the saliva, gastric juice or bile; if it still go on after ligature of the bile and pancreatic ducts, the digestive power must of necessity be referred to the action of the intestinal mucus, provided no change has previously taken place in the stomach; but if the food passes unchanged after cutting off the supply of bile and pancreatic juice, but proceeds after ligature of the bile-duct alone, the act of digestion must be referred to the pancreatic juice.

The author first briefly describes the structure of the starches and starch-containing vegetables employed in his experiments; then the changes produced by cooking, and finally enters on a minute description of the changes observed in the experiments he performed on

normal digestion, and after cutting off the supply of bile and pancreatic juice.

The correct appreciation of the structure of the starch-granule is of considerable importance in relation to these investigations, and the author believes that he has been able to afford a satisfactory solution of this vexed question. The changes observed during the digestion of starch favour the original opinion of Leuwenhoeck, that the starch-granule consists essentially of an investing membrane or cell-wall, enclosing an amorphous matter, the true starch, which strikes an intense blue colour with iodine; and these changes also support the opinion of Professor Quekett, that the concentric circles seen on the starch-granules of many plants are simple foldings of the investing membrane, leaving it still doubtful, however, whether these concentric circles are not in the starches of some plants composed of linear series of dotted elevations or depressions of the investing membrane.

By these experiments it was determined that the concentric circles remain after the whole of the starch matter, colourable by iodine, was removed, and that even then the characteristic cross and colours were still seen when the granules were viewed by polarized light, although more feebly than before; this result being probably due to the lessened power of refracting light, after the removal of the starch matter.

After describing the structure of the wheat-grain and flour, the changes occurring in the wheat-starch during the manufacture of bread are given in detail; but the most interesting of the changes produced by cooking are those seen in the boiled or roasted potato and in the boiled pea.

In each of these the act of cooking effects two purposes:—it causes great enlargement and physical change of the starch-granules, and dissolves the intimate adhesion of the starch-cells, which afterwards appear as ovoid or globular, slightly adherent bodies distended by the swollen starch-granules, the outlines of which are indicated by more or less irregular gyrate lines, produced by the mutual compression of the starch-granules within an inelastic cell-membrane.

The starch-granules of the pea possess a much thicker investing membrane than those of the potato, which causes their outlines to remain much more distinct after the removal of the true starch sub-

stance during the process of digestion. The other structures seen in the pea are carefully described; the most curious among them being the cells composing the external layer of the testa, which bear so strong a resemblance to columnar epithelium of the intestine, that they might be mistaken for the latter by an inattentive observer.

The substances submitted to experiment were,—1, boiled wheat-starch; 2, wheaten bread; 3, uncooked tous les mois; 4, boiled tous les mois; 5, boiled potato; 6, uncooked peas; 7, boiled peas; 8, boiled peas after ligature of the bile-duct; 9, boiled potatoes after ligature of the bile and pancreatic ducts. Several subsidiary experiments were made to determine the action of the intestinal mucus, the saliva, and the substance of the pancreas, on starch.

The conclusions at which the author arrives from the experiments are,—

1. That the starch-granule is composed of two parts, chemically and histologically distinct,— a cell-membrane and homogeneous contents. The markings seen on many varieties of starch are referred to folds or markings of the investing membrane.

2. No perceptible change occurs in the starch, whether raw or cooked, during its sojourn in the stomach of quadrupeds or the ventriculus succenturiatus and gizzard of birds; all the granules preserve their perfect reaction with iodine and their pristine appearance.

3. The conversion of boiled starch into dextrine and glucose is chiefly effected in the first few inches of the small intestine, but it continues to take place in a less degree throughout the entire intestinal canal.

4. In the digestion of boiled wheat or other starch, or of wheaten bread, the bulk of the mass rapidly diminishes in its passage through the small and large intestines, so that it ultimately yields only a small quantity of fæcal matter. After being deprived of their contents, the membranes of the granules shrink and shrivel up into a minute granular matter, which constitutes the chief bulk of the fæcal evacuations after an exclusive diet of starch food.

5. The digestion of raw starch food (peas) in the pigeon or other granivorous birds goes on much more slowly, and progresses pretty equally throughout the entire intestinal canal. The starch-granules,

whether free or included in cells, become intersected by radiating or irregular lines or fissures, more or less opaque or granular; they also gradually lose their characteristic reaction with iodine; and this important change, commencing at the surface, progresses towards the centre, until the whole of the starch matter is removed, leaving the starch-membranes often apparently whole, retaining their characteristic markings. The fissured and granular condition of the starch-granules is not due to their trituration in the gizzard, but to the action of the intestinal fluids, since it was often seen in granules enclosed in and protected by perfect starch-cells. In the digestion of raw starch food, a considerable quantity always escapes change, for many starch-cells and granules in the fæces perfectly retain the characteristic reaction with iodine.

6. As the starch remains unchanged in the stomach, its conversion into glucose cannot be attributed to the saliva or gastric juice, unless we suppose these fluids to remain inactive in the stomach, and suddenly to regain their activity in the first part of the small intestine. The author found that the saliva was capable of effecting the conversion of starch into glucose, but that the mixture of saliva and gastric juice in the stomach did not possess that property even after being rendered alkaline by carbonate of soda. It is probable that the converting power of the saliva, as it flows from the mouth, depends not on the true saliva, but on the buccal mucus; for Magendie found that saliva taken from the parotid duct was wholly inactive, while the mixed saliva from the mouth effected the conversion with great facility. Unless, then, the sublingual and submaxillary glands secrete a different fluid from the parotids, it is evident that the activity of the saliva must be attributed to the buccal mucus.

7. The difference between the digestion of boiled and raw starch in dogs is seen in the experiments on the digestion of boiled wheat-starch, boiled tous les mois, and bread. In all these, some starch-granules escape the action of heat and water, and remain in nearly their pristine condition. These uncooked starch-granules undergo slow and imperfect changes, being fissured, broken, and more or less altered, but in general retaining their characteristic reaction with iodine.

8. The conversion of starch into glucose is not effected by the

bile, since after ligature of the common bile-duct, the changes occur to as great an extent as when the bile passes freely into the intestinal canal.

9. It is not due to the pancreatic juice, inasmuch as after ligature of the bile and pancreatic ducts in the same animal, the digestion of starch is still effected.

10. The only remaining secretion is the intestinal mucus, which is especially abundant at the upper part of the intestinal canal; and a further proof is afforded of the activity of the intestinal mucus taken from the upper part of the duodenum above the entrance of the pancreatic duct after ligature of this duct and the common bile-duct, by its capability of converting a large quantity of fresh boiled starch into glucose out of the body.

11. In the cooking of starch-containing vegetables, such as potatoes and peas, the adhesion of the starch-cells is dissolved or weakened so as to render them easily separable and amenable to the action of the intestinal fluids. At the same time the starch-granules undergo a large increase in bulk, distend the cells, and by their mutual compression, their outlines present the appearance of gyrate lines beneath the cell-wall. The cells seldom burst so as to emit their contents, or present any appreciable opening through which the intestinal fluids can directly penetrate. The author cannot positively affirm so much of the starch-membranes, because these are so extremely delicate that fissures might be invisible, but he believes that in a great number the membranes remain entire.

12. If this be the case, the conversion of starch matter into glucose must be effected by the permeation or endosmose of the intestinal fluids through the invisible pores of two membranes, in the digestion of the pea, the potato, and other similar foods, and the glucose must escape through the same membranes by exosmose.

13. Before the conversion of starch into glucose, the amylaceous matter contained in the starch is more dense than the intestinal mucus in immediate contact with the cells, and an inward current or endosmose is established, but after that conversion the syrupy fluid is less dense than the mucus, and then an outward current or exosmose occurs, by which the glucose escapes from the cells into the intestine and is absorbed. If this be the case, as the details of

the experiments tend strongly to prove, a new and important function is assigned to the intestinal mucus.

14. In normal digestion, chyme escapes very slowly from the stomach into the duodenum, in small quantities, as it is detached from the alimentary mass by the muscular movements of the stomach, and this gradual propulsion often occupies several hours after a meal. This slow propulsion is evidently intended to expose the comminuted food fully to the action of the intestinal juices, and produce an intimate mixture with them. The comparatively empty condition of the upper part of the small intestine, even during active digestion, is thus fully explained.

15. If the food be too finely divided or incapable of a second solidification in the stomach, it passes too rapidly into the first part of the small intestine, is insufficiently mixed with the intestinal fluids, and a considerable part escapes digestion. On the other hand, if it enters the small intestine in masses incapable of reduction by the muscular action of the parts or solution in the fluid, it traverses the intestinal canal unchanged, except at the surface, which is then alone exposed to the action of the intestinal fluids.

16. It is not necessary for the conversion of starch into glucose that the fluids in the duodenum or other parts of the intestinal canal should be alkaline, or even neutral, for in several of the experiments the contents of every part of the alimentary canal had an acid reaction.

17. The greater part of the intestinal mucus is not excrementitious, for little, if any, mucus is perceptible in the fæces in normal digestion, except at their surface, whereas the greater proportion of the contents of the small intestine consists of mucus. A considerable quantity of mucus is seen in the cæcum, but it rapidly diminishes in the colon, and is scarcely detectible in the fæces, except that on the surface, which is probably derived from the mucous membrane of the rectum. The author raises the question, whether one of the chief functions of the cæcum is not to effect the conversion of the intestinal mucus into some other substance capable of re-entering the blood, and performing some ulterior purpose in the animal economy.

18. In normal digestion, the separation of the epithelium of the mucous membrane of the intestine is the exception instead of the rule,

as stated by some physiologists. The author questions the theory of the detachment of the epithelium of the villi in each act of absorption, on the grounds that the presence of detached epithelium was unfrequent in the whole course of his experiments; that epithelium is readily detached by manipulation; that the continual reproduction of such a vast amount of cell-tissue must necessarily be accompanied by a vast expenditure of vital force; and finally, that it is not necessary, because fluids readily penetrate epithelial membranes.

19. The passage of a given food through the whole length of the intestinal canal may occupy a comparatively short time, especially when the animal is fasting. In one experiment, where a pigeon refused food until the fæces contained no visible debris of previous food, starch-granules were detected in the fæces within two hours after a meal, and this although the intestine of this animal is extremely narrow and about a yard in length.

20. A remarkable circumstance in the digestion of starch or starch foods is the constant presence of myriads of vibriones in the lower part of the intestinal canal. They are generally first observed in the lower part of the small intestine, as minute brilliant points, just visible with a power of 600 diameters, in active movement. They increase in numbers towards the cæcum, in which a large number of fully-developed vibriones are constantly seen. These minute organisms increase in size and length in the colon and rectum, and their fissiparous mode of propagation, first described by the author in the 'Quarterly Journal of Microscopical Science,' may be distinctly traced by examining the contents of these portions of the intestine.

February 8, 1855.

The LORD WROTTESLEY, President, in the Chair.

A paper was in part read, entitled "An Account of some recent Researches near Cairo, undertaken with the view of throwing light upon the Geological History of the Alluvial Land of Egypt."—Part First. By LEONARD HORNER, Esq., F.R.SS. L. & E., F.G.S.

February 15, 1855.

THOMAS BELL, Esq., V.P., in the Chair.

Edward John Littleton, Baron Hatherton, was balloted for and duly elected a Fellow of the Society.

The following communications were read:—

I. The reading of Mr. HORNER's paper was resumed and concluded.

The author commences by observing, that although it be highly improbable that we can ever form an appropriate estimate in years of the age even of the most modern strata, we are not cut off from all hope of being able to assign an amount in years to the duration of some of the great geological changes which, in past ages, the present surface of the earth has undergone, by causes that are still in operation; especially by a careful study of the formation of the deltas of great rivers, and of the action of the latter on the rocks and soils they traverse in their course. If in a country in which a certain alteration in the land has occurred, we know that such alteration has taken place in part within historical time, and if the entire change under consideration presents through-

out a tolerable uniformity of character, we may be justified in holding the portion that has taken place within the historical period to afford a measure of the time occupied in the production of the antecedent part of the same change.

Egypt supplies us with the earliest evidence of the existence of the human race recorded in works of art; in its monuments we find the dawn of the historical period and of civilization; and that land alone, of all parts of the world as yet known to us, offers an instance of a great geological change that has been in progress throughout the whole of the historical period, in its annual inundations and the sediment these deposit to form the alluvial land in the valley of the Nile; and there is good reason for believing that the change had been going on with the same uniformity for ages prior to that period when our reckoning of historical time begins. To investigate the formation of the alluvial land in the valley of the Nile in Upper and Lower Egypt is therefore an object of the highest interest to the geologist and the historian.

The author being impressed with the conviction that this geological problem could only be solved by having shafts and borings made in the alluvial soil to the greatest practicable depths, determined to have some such experiments made; as the results might lead the way to other researches on a greater scale. The ground on which he hoped to be able to form a chronometric scale by which the total depth of sediment reached might be measured, was the same as that on which the French engineers in 1800 had proceeded, viz. the accumulation of Nile sediment around monuments of a known age. If that depth of sediment be divided by the number of centuries that have elapsed since the date of the erection of the monument, we obtain a scale of the secular increase of which the base of the monument is zero, assuming that the average increase from century to century has been uniform within an area of some extent. Then if the excavation be continued below the base stone, and the sediment passed through exhibits similar characters as to composition with that above the base line of the monument, it would be fair to apply the same graduation below the zero-point of the scale as above it, and, if we reached so far, we should be able to estimate the time that has elapsed since the first layer of sediment was deposited on the rock forming the channel over which the water spread when it first flowed northward from its sources in the interior of Africa,

subject however to correction for causes that might make a difference in the rate of increase between the earlier and later periods.

The author submitted his scheme to the President and Council of the Royal Society, who encouraged his proceeding by acceding to his request of a grant from the Donation Fund at their disposal, towards the expenses of the researches.

The author introduces his subject by a sketch of the physical geography and geology of Egypt, a description of the annual inundations, and of the sediment deposited from the water of the Nile.

Egypt is separated from Nubia by a low hilly region, about fifty miles broad from north to south, chiefly composed of granitic rocks, but associated with two kinds of sandstone, the one belonging to the cretaceous series, the other of the newer tertiary age. The valley of Upper Egypt is bounded by two ranges of hills running northward, the Arabian range on the right, the Libyan on the left of the river, both alike composed of sandstones and limestone. The cretaceous sandstone extends from the granitic rocks forming the first cataract at Assouan for about eighty-five miles, where it is covered by a limestone which has the characters of the upper chalk of Europe. This chalk continues on both sides of the valley for about 130 miles, when it is covered in its turn by a tertiary nummulite limestone, and of which the further prolongation northward of both ranges is composed; this nummulite limestone can be well studied in the extensive quarries of Gebel Mokattam above Cairo.

The author briefly describes the operations of the private association of English, French, and Austrian Engineers in 1846–47, commonly called the French Brigade, for the purpose of determining the disputed question of the relative levels of the Red Sea and Mediterranean. The French engineers, at the beginning of the present century, had come to the conclusion that the Red Sea was about 30 feet above the Mediterranean, but the observations of Mr. Robert Stephenson, the English engineer, at Suez, of M. Negretti, the Austrian, at Tineh near the ancient Pelusium, and the levellings of Messrs. Talabot, Bourdaloue and their assistants, between the two seas, have proved that the low-water mark of ordinary tides at Suez and Tineh is very nearly on the same level, the difference being, that at Suez it is rather more than one inch lower.

At the island of Philæ, about five miles above Assouan, may properly be placed the first entrance of the Nile into Egypt. It is here

about two miles broad, but is soon after divided into several branches by the rocks that rise up in its bed to form the rapids, commonly called the First Cataract, which have a descent of about 85 feet in a distance of five miles. Here the river is contracted to about a third of a mile. Assouan is about 300 feet above Cairo, and the distance between the two places being 556 miles, the average fall of the river is little more than half a foot in a mile, 0·54, and Assouan being 365 feet above the Mediterranean, and 696 miles distant from it, the average fall of the Nile from the foot of the First Cataract to the sea is 0·525 in a mile. Low Nile at Cairo is 43 feet $6\frac{1}{2}$ inches (as measured by the French Brigade in 1847) above low-water mark in the Mediterranean, and the distance being 149 miles, the average fall is little more than $3\frac{1}{2}$ inches in a mile. The author cites a report of Mr. Rennie to the British Association in 1834, showing that the fall of the Thames, between Chertsey and Teddington Lock, is nearly $17\frac{1}{2}$ inches in a mile.

The commencement of the annual inundation is about the summer solstice. The rise is scarcely perceptible for six or eight days, it then becomes more rapid, and about the middle of August has usually reached one-half of the greatest amount; it attains its maximum towards the end of September, remains pretty stationary for about fourteen days, and then begins to fall, at first at a more rapid rate than that with which it rose, but after it has fallen one-half the decrease is very gradual, and it goes on sinking until the end of May. The rise continues about 90 days, the falling lasts 250. In 1846, Mougel Bey, the French engineer of the Barrage near Cairo, found the maximum rise 7·20 metres, or 30 feet 10 inches.

When the inundations commence, the Nile is of a reddish colour, and is loaded with sand and mud. From the fall between the Second Cataract at Wadi Halfa and the First, a distance of 214 miles, being not more on an average than 9 inches in a mile, very little coarse gravel can be transported by the river into Egypt. The greater portion of the heavier detritus falls down in the higher parts of Upper Egypt, and from the very gentle slope of the Delta, only a small amount of the solid matter suspended in the water can reach the sea; still, however, the sea has been observed to be turbid at a distance of forty miles from the mouths of the Nile.

The author then proceeds to describe the recent researches. His

first and indispensable step was to procure the aid of a competent person to conduct his projected operations, and he was fortunate in obtaining the active and intelligent aid of an Armenian gentleman, educated and long resident in England, Hekekyan Bey, a civil engineer, who had occupied some important positions in the service of the Viceroy Mehemet Ali, especially as chief of the Polytechnic School in Cairo. But nothing could be done without the consent of the then Viceroy Abbas Pacha, more especially as the spot the author had selected for his first operations was in a garden of the Pacha. By the active intervention of Her Majesty's Consul-General in Egypt, the Honourable Charles Augustus Murray, not only was the Viceroy's consent obtained, but his Highness was pleased to direct his ministers to place at the disposal of Hekekyan Bey whatever was necessary to conduct the operations in the most complete manner; and, with truly royal munificence, ordered that the whole expense of them should be defrayed by his Treasury. The author never contemplated having the means of making these researches on more than a very limited scale, but he had now the prospect, and it has since been realized, of their being conducted on a very enlarged plan.

It has been often a subject of regret that experiments of this nature, of which the French had set an example half a century ago, had not been followed up. On this subject the author remarks, that the operations are of a nature that scarcely any individual traveller could undertake; for they require a large body of men, some practised in the art of surveying, and as they can only be carried on continuously after the inundation waters have subsided for some time, and therefore at a season of the year when the heat is excessive, those only inured to the climate could stand the work.

The place selected for commencing the operations was at the Obelisk of Heliopolis, about six miles below Cairo, the oldest known; erected, according to Lepsius, 2300 years before Christ. The author having given Hekekyan Bey full and minute directions as to the manner in which the researches were to be carried on, the observations to be made, the plans and reports to be drawn up, and the specimens of soils sunk through to be selected, the operations commenced in June 1851. Sixty men were employed under the direction of Hekekyan Bey, assisted by an officer of Artillery, and some young engineers from the Polytechnic School in Cairo.

Nine pits or excavations were sunk at different distances around the obelisk, each down to the level of the filtration water from the Nile at that season, and as much under the surface of that water as was practicable. The most important of these was one close to the obelisk. The upper surface of the pedestal on which the obelisk rests, was reached at the depth of 5 feet 6 inches below the surface of the ground, Nile mud being accumulated to that height; the pedestal was 6 feet 10 inches in height, and it was found resting on two limestone flags, the upper 16 inches, the lower 15 inches in thickness, and this foundation was laid upon pure quartzose sand. This last was penetrated to the depth of 3 feet $2\frac{1}{2}$ inches below the lower layer of limestone.

The author gives a section and description of each of the nine excavations. But before doing so, he states that he obtained twenty-eight specimens of soils sunk through in different parts of the Nile Valley, eleven of which were carefully analysed at the Royal College of Chemistry, under the superintendence of Dr. Hofmann. A collection of specimens, duplicates of which are in the possession of Hekekyan Bey, serve as a standard for the description in his reports of the soils passed through, to avoid the necessity of sending specimens of identical alluvia. These samples were carefully compared with the specimens analysed, and were found to resemble them closely in external characters.

The results of the analyses are given by the author, and the average of eight specimens of Nile mud gives the following composition in 100 parts:—

Silica	54·585
Sesquioxide of iron	20·215
Sesquioxide of alumina	6·418
Alumina	5·237
Carbonate of lime	3·717
Sulphate of lime	0·245
Lime	1·912
Magnesia	0·762
Potassa	0·473
Soda	0·553
Organic matter	5·701
	99·818

In order to ascertain the amount of solid matter held in suspension in the water of the Nile near Cairo, the author described to Dr. Abbott, a resident in that city, the method he had followed in 1832 to determine the amount of solid matter suspended in the water of the Rhine at Bonn, and requested him to undertake the experiment on the same plan, which he did, and the result gave 110·6 grains in an imperial gallon. The residuum sent was analysed at the Royal College of Chemistry, and yielded very nearly the same result as to composition as the above average analysis of the Nile sediment.

On examining the descriptions of the soils sunk through in the nine excavations at Heliopolis, it appears that they consist of two principal kinds, viz. earths and sands. The earths vary in colour, but are so nearly allied, passing by such insensible shades into each other, and having so great a resemblance to the modern Nile sediment, that they may all be classed as Nile mud. The sands are almost entirely pure quartz, similar to those of the adjoining deserts.

In the same horizontal plane, even in this limited space of half a square mile, there is a very considerable difference in the nature of the soil, and in none of the excavations was there an instance of lamination in the deposit.

When it is considered how small is the amount of sediment left annually by the inundations in any one place, it is very difficult to conceive, in the author's opinion, how there should be in any one spot so great a thickness as 12½ feet of one kind of sediment, as is the case in one of the excavations, without any lamination or other sign of successive deposition, and still more inconceivable that in pits within a very short distance of each other different kinds of soil should be found at the same levels. Other causes than the tranquil deposit from inundation water must have been at work in the formation of this portion of the alluvial land. The layers of sand were most likely blown across the valley from the desert.

The author deems it advisable to abstain from general remarks, and from all inferences as to the secular increase of the alluvial deposits, until he has had an opportunity of laying before the Society an account of the far more extensive researches made in the district of Memphis in 1852, and during the last year in a series of pits sunk in a line across the valley of the Nile, extending from the Libyan to the Arabian Chain, in the parallel of Heliopolis.

In the various excavations that have been made in the prosecution of this inquiry, many objects of art of historical interest have been discovered; but as these do not come within the province of the Royal Society, the author proposes to give an account of them in a memoir to be laid before another learned body.

The following communications were read :—

II. "On the Computation of the Effect of the Attraction of Mountain-masses, as disturbing the apparent astronomical latitude of stations in Geodetic Surveys." By GEORGE B. AIRY, Esq., F.R.S., Astronomer Royal. Received January 25, 1855.

The author commences with remarking that his surprise had been excited by the result obtained by Archdeacon Pratt*, namely, that the computed attraction of the elevated country north-east of India considerably exceeds the disturbance which it was sought to explain. But on consideration the author perceived that this result might have been anticipated, on the extensively received supposition that the interior of the earth is a dense fluid or semi-fluid (which for convenience he calls *lava*), and that the exterior crust floats upon it. For, he remarks, this crust cannot be supposed at any part to be very high upwards (as in mountains), at least to any great horizontal extent, unless there is a corresponding projection downwards into the lava. Upon making a numerical calculation, even with the crust 100 miles thick, it was shown that there would be such a tendency of the table-land to crack and sink in the middle as no cohesion of rocks can resist. He conceives that the state of the ground may be properly illustrated by a raft of timber floating on water: if one piece of timber projects higher into air than the others, we are certain that it also projects lower into water than the others. Assuming this as established, then it is evident that the horizontal attraction of a mountain-mass on a point at a considerable distance is nearly evanescent, because the increase of attraction of the part which is

* Proceedings of the Royal Society, December 7, 1854.

above the general level is sensibly neutralized by the deficiency of attraction below it where the lighter crust displaces the heavier lava. In like manner, the horizontal attraction of a ship or other floating body is nothing. But the horizontal attraction upon a near point on the earth's surface will not vanish, because the mountain which produces the positive attraction is nearer than the lava-displacement which produces the negative attraction: even here, however, the efficient disturbing attraction will be much less than that computed by considering the dimensions of the mountain only.

III. Note to a paper entitled "Contributions to the Anatomy of the Brachiopoda," read June 15, 1854. By Thomas H. Huxley, Esq., F.R.S. Received February 12, 1855.

My attention having been called within the last two or three days, o an error in my paper on the Anatomy of the Brachiopoda, published in No. 5 of the Royal Society's Proceedings, I beg to be allowed to take the earliest opportunity of correcting it. At p. 111 of that paper the following paragraph will be found:—

"In 1843, however, M. Vogt's elaborate Memoir on *Lingula* appeared, in which the true complex structure of the 'heart' in this genus was first explained and the plaited 'auricle' discriminated from the 'ventricle;' and in 1845, Professor Owen, having apparently been thus led to re-examine the circulatory organs of the Brachiopoda," &c. &c.

Now, in point of fact, though M. Vogt *does* describe and accurately figure the structures called 'auricle' and 'ventricle' in *Lingula**, yet he has not only entirely omitted to perceive their connexion, or to indicate the 'auricular' nature of the former, but he expressly states that the so-called 'hearts' are "simple, delicate, pyriform sacs" (p. 13).

I presume that my recollection of M. Vogt's figures was more vivid than that of his text; for having been unable, notwithstanding repeated endeavours, to re-obtain the memoir when writing my paper,

* Neue Denkschriften der allgemeinen Schweizerischen Gesellschaft für die gesammten Naturwissenschaften. Band VII.

I felt justified in trusting to what seemed my very distinct recollection of its sense. I had the less hesitation in doing this, as in M. Vogt's subsequently published 'Zoologische Briefe *,' he gives the received interpretation to the parts of the so-called 'hearts' without any indication of a change of opinion.

I make this statement in explanation of what might otherwise seem to be great carelessness on my part, and for the purpose of further pointing out that M. Vogt not having made the supposed discovery, it is quite impossible that Professor Owen's researches should have been suggested by it.

February 22, 1855.

The LORD WROTTESLEY, President, in the Chair.

Henry John Reynolds Moreton, Earl of Ducie, was balloted for and duly elected a Fellow of the Society.

The following communications were read:—

I. "On the Temperature and Density of the Seas between Southampton and Bombay *viâ* the Mediterranean and Red Seas." By MM. ADOLPHE, HERMANN, and ROBERT SCHLAGINTWEIT. Communicated by the Court of Directors of the Honourable East India Company. Presented by Professor STOKES, Sec. R.S. Received January 11, 1855.

In this communication the authors give the results of the observations they had made during their voyage, relative to the temperature and specific gravity of the sea-water, both near the surface and at depths ranging from about 18 to 30 metres, the latter being nearly

* Frankfort, 1851, vol. i. p. 285.

the greatest depth which the motion of the vessel permitted them to reach. They reserve for a future report their observations on the temperature and moisture of the air, as well as the results of two experiments on the quantity of carbonic acid contained in the air on the Mediterranean and Red Seas.

The instruments employed in the observations here described were as follows :—

(1.) Four thermometers which had been carefully compared at the Kew Observatory previous to the authors' departure. At Bombay they repeated the determination of the zero-point and of another standard point, and found that the thermometers had not varied.

(2.) A dipping apparatus constructed by Mr. Adie. This apparatus, which held 5 or 6 litres, was furnished with two valves, so arranged that as it descended the water passed freely through, but as soon as a commencement was made of drawing it up the valves closed and rendered it water-tight. The authors assured themselves that the temperature of the enclosed water did not sensibly change during the process of drawing it up.

(3.) An areometer from Mr. T. G. Greiner at Berlin. This instrument permitted the specific gravity to be read off directly to three places of decimals, and the fourth could be supplied by estimation.

To render the observations of specific gravity comparable with one another, it was necessary to reduce them to a common temperature, which occasioned some difficulty, as the exact expansion of sea-water between the limits 20° and 25° C. was not accurately known. By means of a delicate voluminometer, constructed for the purpose by M. Geissler of Berlin, the authors determined the expansion to be 0·000271 for 1° C. For distilled water Halström had found 0·000219. Another set of more direct experiments, made at Bombay, gave for the expansion of sea-water 0·000337. The difference between this value and the preceding the authors refer to a change of volume of the voluminometer itself, and they prefer the latter, which accordingly they use in their reductions.

The authors then give tables of the results of their observations, which are followed by some general remarks.

Atlantic.—The temperature of the Atlantic was found to be—

From lat. 46° to 41° N. .... 17°·5 to 18°·5 C.
From lat. 39° to 37° . ... 20° to 21° C.
Mean specific gravity reduced to 17°·5 C.=1·0277.

The temperature and specific gravity showed very little variation in the open sea, so long as no currents were met with, but in the vicinity of land, disturbances of various kinds were noticed. In harbours and in small bays the temperature of the water was found to diminish sensibly at a depth of from 15 to 20 metres, but in the open sea the temperature at the very surface was generally found somewhat lower than at a depth of 30 metres, which no doubt was due to evaporation.

Straits of Gibraltar.—A current with a mean velocity of from three to six miles an hour usually flows through the Straits from the Atlantic into the Mediterranean. A counter current is supposed to exist underneath, but the great depth of the Straits prevented the authors from reaching any such current with their dipping apparatus.

East of the Straits the water of the Atlantic was met with in several places, in close proximity with water of the Mediterranean, from which it was distinguished by its temperature and colour. The stream from the Atlantic on passing the Straits seems to divide itself into several branches.

In connexion with the variability of the currents in the Straits, it is worthy of remark that the *unreduced* specific gravity of the water of the Mediterranean and of the Atlantic is nearly the same.

Mediterranean :—

From the Straits of Gibraltar to Malta—

Temperature of the water .... 21°·7 to 22° C.
Specific gravity reduced to 17°·5 C... 1·0287.

From Malta to Alexandria—

Temperature .... 23° to 24° C.
Reduced specific gravity .... 1·0298.

Red Sea.—The maximum of specific gravity found during the

voyage was in the northern end of the Gulf of Suez, 1·0393 (reduced).

From lat. 27° to 23° N., temperature 24° to 28° C.: reduced sp. gr. 1·0315.
From lat. 22° to 14° temperature 30° to 31°·5 C.: reduced sp. gr. 1·0306.

Straits of Bab-el-Mandeb.—The water of the Gulf of Aden being less dense, though colder, than that of the Red Sea, flows into the latter on both sides of the Island of Perim. This colder water could be detected half a degree north of the Straits. After some further remarks about this current, the authors pass on to the

Arabian Sea—in which they found

From long. 44° to 50° East from Greenwich, temp. 28°·8, reduced sp. gr. 1·0275.
From the merid. of Cape Guardafui to Bombay, temp. 27° to 28°, red. sp. gr. 1·0278.

II. A paper was in part read, entitled "On the Structure, Functions, and Homology of the Manducatory Organs in the Class Rotifera." By P. H. Gosse, Esq. Communicated by Thomas Bell, Esq., V.P.R.S. Received January 5, 1855.

November 30, 1854.

ANNIVERSARY MEETING.

The EARL of ROSSE, President, in the Chair.

Mr. Paget, on the part of the Auditors of the Treasurer's Accounts, announced that the total receipts during the past year, including a balance of £1006 17*s.* 7*d.* carried from the account of the preceding year, amounted to £4252 12*s.* 0*d.*, and that the total expenditure, including an investment of £800 in the Funds, was £3208 12*s.* 3*d.*, leaving a balance in the hands of the Treasurer of £1043 19*s.* 9*d.*

The thanks of the Society were voted to the Treasurer and Auditors.

List of Fellows deceased since the last Anniversary.

On the Home List.

His Majesty, Frederick, King of Saxony.

James Andrew, LL.D.
Golding Bird, M.D.
William Blake, Esq.
William Brockedon, Esq.
Captain Crozier, R.N.
William, Earl of Dartmouth.
Hart Davis, Esq.
Thomas, Lord Denman.
General Sir Benjamin D'Urban.
Edward Forbes, Esq.
Admiral Sir John Franklin.
John Davies Gilbert, Esq.
Rev. Samuel Gardiner.
Henry Harvey, Esq.
John Harwood, M.D.
Charles Hoare, Esq.
Sir Everard Home, Bart.
Robert Jameson, Esq.
Sir Richard Jenkins.
John Augustus Lloyd, Esq.
Lieut.-Col. Mudge.
George Newport, Esq.
George Roupell, M.D.
Charles Stokes, Esq.
George Townley, Esq.
Charles Baring Wall, Esq.
Nathaniel Wallich, M.D.
Alexander Wollaston, Esq.

On the Foreign List.

Baron Von Lindenau.
Macédonie Melloni.
Charles François Brisseau Mirbel.

Withdrawn.

Captain Chapman, R.A.

Defaulters.

E. B. Beaumont.
E. W. Tuson.

List of Fellows elected since the last Anniversary.

George James Allman, M.D.
William Bingham Baring, Lord Ashburton.
Edward William Brayley, Esq.
Alexander Bryson, M.D.
J. Lockhart Clarke, Esq.
Joseph Dickinson, M.D.
Ronald Campbell Gunn, Esq.
Robert Hunt, Esq.
John Bennet Lawes, Esq.
Robert Mallet, Esq.
Charles May, Esq.
Capt. Thomas E. L. Moore, R.N.
Captain Richard Strachey.
Robert Dundas Thomson, M.D.
Samuel Charles Whitbread, Esq.
Wm. Crawford Williamson, Esq.

The President then addressed the Society as follows:—

Gentlemen,

When we met last November, I ventured to remark that the objects of science were better understood than they had been formerly, and that accurate notions on scientific subjects were becoming more prevalent: the progress in that direction has still continued, and during the last year it has been even more decided than before.

With the spread of knowledge, the great body of the community are now better enabled to appreciate the importance of science in promoting the moral and physical improvement of mankind; there is consequently a growing desire that science should be advanced, and as that object has been best effected by scientific societies, they are regarded with more interest. Public opinion having taken that direction decidedly, it was not unreasonable to expect that Government would feel anxious to meet the wishes of the scientific bodies, and provide them with a building where they would be enabled to

employ to the best advantage the machinery of association, which had already effected so much. It is probable, therefore, that before very long you will have the important question to decide,—whether to retain your present apartments, or to accept a suitable place in a new building. Throughout all the communications with the Government, your officers have taken especial care to be perfectly explicit on two points: one, that till the plans were prepared, any final decision on your part was impossible; the other, that you had a free option to retain your present apartments should you think fit, having received them as a grant, to endure so long as the Society should exist. There can now, I think, be little doubt but that the leading Scientific Societies will be suitably provided for, and that you will have the opportunity of taking your proper place at the head of science, with the other Societies by your side.

The pursuit of science will thus be greatly facilitated; it will be rendered more convenient, and therefore more attractive; and many will devote themselves to it who perhaps otherwise would not have entered upon it at all.

While, however, the Government is wisely anxious to encourage the active pursuit of science by meeting the wishes of the great body of scientific men, we perhaps may have it in our power to effect something in the same direction.

From various observations of my late lamented predecessor in his Addresses, it was evidently his opinion that we should act wisely in shaping our rules so as to adapt them to the varying usages of society, rather than to preserve everything unchanged, relying on the sanction of a long prescription. In former times, November was the height of the London season, and the Anniversary, with all its preliminary business, was naturally held then, because the largest number of Fellows were in town. Whether for the better or not, the season has been changed, and it is now six months later; we, however, remain where we were. The Council meets the latter end of October, and continues its meetings through November; the principal business being to award the medals, and to select the Officers and Council to be recommended to the Society at large for election. The November business is wound up by the Anniversary, the most important meeting of the Society. The Fellows who are not permanent residents in London are naturally absent in the

country: the great business of the year, with all its responsibilities, devolves therefore upon a section of the Society. A country gentleman, if named upon the Council, cannot conveniently attend in November, and but few country gentlemen attend the anniversary*. Our proceedings are therefore not of as much interest to the Fellows taken as a whole as they might be made to be, and they are not calculated to attract the men who, having attained high scientific distinction at the Universities, reside in the country, unharassed by professional calls, and who have therefore both training and leisure, important elements of success in all scientific pursuits.

The award of the medals at a time when so many Fellows are absent is also attended with inconvenience; for although an attempt is made to secure for each science a kind of representation in the Council, still so wide is the range of science now, that special departments are often necessarily unrepresented. In disposing of papers, the imperfect representation of individual sciences is unattended with inconvenience, because each paper is referred to two Fellows to report upon; the Council thus calls the whole Society to its aid, and the result, I believe, is perfectly satisfactory. With the medals it is otherwise; no official reference is made to Fellows not on the Council. There is a further difficulty. The questions which usually arise are of this nature. Discoveries have been made by different individuals in various sciences. Who has added most to the general stock of knowledge by a positive contribution? Who has the merit of having effected discoveries of most promise? Recollect, that in answering these questions some estimate must be made of the weight due to each science, for they cannot be considered all alike; very far from it. Some sciences require great mental labour, guided by faculties of a very high order,—a rare gift; while other sciences can be cultivated successfully by common-place men, with only a moderate amount of perseverance. That such an estimate can be made, and which carries with it a kind of general assent, is evidenced by the fact that it is annually made, to some extent, at the examination for the Fellowship in the University of Dublin, which bears a certain loose but not uninstruc-

* At the last Anniversary the Fellows who voted amounted to 63, while there are about 700 Fellows. I am informed, however, that usually several who are actually present do not vote.

tive analogy to the process by which your Council awards the medals. That examination is the most difficult which exists; the prize is large, and the competition is free. There is a course of mathematics, comprehending, I may say, everything; a course of physics equally large; a very large course of moral philosophy; a course of metaphysics, of logic, classics, history, chronology, and Hebrew. The examination is, of course, public; and a person of experience, acquainted with the course, can usually at the close of the examination point out the successful candidate. Some have answered better in one science, some in another, but acting under the guidance of a mature judgment, a kind of equitable adjustment has been made by the bystander, which has led him to the same conclusion as the examiners. Now, let us see for a moment how this has been brought about. The examiners, who are Fellows, are conversant to a certain extent with all the sciences; and in measuring the value of each answer, they are governed by a well-marked public opinion in the University, precisely as is the case with the enlightened audience; and they come to the same conclusion. But with your Council the case is necessarily very different. However chosen, they cannot have within themselves the same means of discharging their very difficult duties in a way which will carry with it the full concurrence of the Society. Take as an example the simplest case which can arise: two persons have been proposed for the medal, a chemist and a mathematician. Upon the Council we will presume there is a first-rate chemist and a first-rate mathematician. Now, in chemistry and in mathematics, and indeed in all the sciences, little discoveries are very abundant. By what possible means can the chemist bring his mind to bear upon the little discoveries of the mathematician, so as to weigh them, even in the roughest manner, against the discoveries in his own science? Or will the mathematician be more fortunate in dealing with the discoveries of the chemist? But how is it with respect to the other members of the Council? There will probably be gentlemen representing the different branches of the natural sciences, also perhaps a geologist, an astronomer, an engineer. Why, even in the very simple case I have supposed, the elements for the roughest approximation to a true conclusion are not within the Council, and it cannot be otherwise. The Council must therefore

travel beyond its limits for the necessary information. Individual members will naturally therefore, in an unofficial way, consult such Fellows not on the Council as are known to be conversant with the particular question at issue; and, practically, if time permitted and opportunity offered, a large proportion of the ablest Fellows would exercise a guiding influence on the Council, leading them to correct conclusions. Information would even be sought without the Society; the prevalent opinions at the Universities would be looked for, and the state of public opinion abroad in scientific circles would in many cases have great weight. I need hardly say, that as things are at present, this inquiry is impossible except to a very limited extent, and public opinion, even in our own body, can afford but little of that aid to the Council which it would do under more favourable circumstances.

If the medals were awarded in June, after discussion in several successive Councils at considerable intervals, while the great body of Fellows, the leading members of the Universities, and the foreigners who visit London, were in town, each member of the Council would have immediate access to the best sources of information. Recently the experiment has been tried of proposing candidates for the medals before the recess, but without, I think, any practical advantage. Where the candidates are numerous, inquiries would be endless; and it is only when the number has been reduced, when the doubtful questions have been put prominently forward by discussion, and a decision is imminent, that inquiries will be prosecuted with energy, and can be made with effect.

Finally, experience has shown that even in the transaction of the business of the nation, there is so much inconvenience in running counter to the habits and usages of society, that it is only in a case of necessity that Parliament is assembled in November.

With these or similar views, the subject was brought before your Council in 1845; and, as was announced by Lord Northampton, it was resolved to change the day of the anniversary to a season more generally convenient. In his Address the year after he states that doubts had arisen as to the legality of the change without a new charter, and no serious effort appears to have been subsequently made to surmount the difficulty.

I believe it is well known that Sir Humphry Davy's opinions on this subject were very similar to Lord Northampton's, and I have heard that he had deliberately committed them to paper, setting out fully his views as to the prospects of science in this country, and the position the Royal Society should hold. Such a document would be of great value, and I have anxiously inquired for it, but in vain.

This was the state of things at the time of my election as President, and after full consideration I took the first opportunity, at the anniversary in 1849, of expressing my entire concurrence in the views of Lord Northampton. It appeared to me, however, to be quite evident that there was not a strong and universal desire to change the day of the anniversary; and that was to be expected. There are certain associations, hallowed by time, to which we all recur with pleasure: when we meet on the 30th of November, our thoughts are led back to the auspicious day when the Royal Society was founded: we are reminded of Boyle, Wren, Hooke and Wallis, the first Fellows, and feel a just pride that we enjoy the high honour of being their successors. Few, perhaps, would assent without some degree of reluctance to a change which would sever these ancient associations. Feeling assured, therefore, that there were various shades of opinion, and having stated my views distinctly in my first address, I did not conceive it to be my duty to proceed further. The next step would have been, to have directed the attention of the Council to its former decision as to the expediency of changing the anniversary, and to have obtained the best advice as to the means to be taken to effect that object. I thought it better to let the matter rest for a time. Had I thought otherwise, there was certainly no one by whom such a question could have been brought forward with less propriety, or less advantage, than your President. He could not have recommended his arguments as springing from an unbiassed mind, when he was in the position, not a very agreeable one, of being necessarily absent during the autumn and winter, when there was so much business of importance. Now the case is different. There is no longer any reason for reserve, and in expressing in my last address the same opinions as in my first address, I have ventured to express them more strongly, because experience has more fully confirmed them. If we are to distribute medals, it is

surely important that the award of the medals, like the award of the Fellowships in the University of Dublin, should carry with it the full approbation of the whole Society. If we are to have meetings to transact business, it is of the highest importance to make them easily accessible to all Fellows by the choice of a suitable season, so that every person, whether on the Council or not, might have the opportunity of exercising his privileges with the least amount of personal inconvenience. It is in this way we shall render the Royal Society even more popular than it is, and hasten its growth in strength and influence, so that it will become, not the Royal Society of London merely, but the Royal Society of the whole kingdom.

I have thus ventured to suggest certain changes as to the time and manner of transacting the business of the Royal Society: the objects we have in view would, I think, be much promoted by another innovation, which it requires some courage to propose. I mean a large increase in the number of the Council. From what I have observed, I am convinced that to enable the Council to exercise an effectual supervision over all the sciences, it is necessary to make ample room, so that each science should be fully represented. There is another object perhaps of even greater importance to be attained by the same means, an *effectual representation of all classes* upon the Council, so that men of general attainments should have their place, and the government of the Society should not be exclusively entrusted to men, who, however eminent in especial branches of science, may not be always the most conversant with worldly affairs, or the most competent to transact that common-place business, upon which, in the main, the prosperity of Societies depends: nothing would do more than such a change, to promote harmony and good feeling within our walls; nothing would contribute more to increase the influence of the Royal Society in advancing the general interests of science.

I have already ventured to say, that I had little doubt but that the memorial with its two hundred signatures in favour of juxtaposition, backed up by public opinion, would produce the desired result, and that before long we should see the leading scientific Societies under the same roof. I think I cannot now err in expressing a confident belief, that whatever changes may be required in our Society to meet the just wishes of scientific men, will be carried out with

readiness. The progress of science will thus be promoted, and this country will gradually attain even a higher place in European science than that which it at present holds. There is a new quarter, however, to which science may I think look hopefully, and that is the University of Oxford. Last session an act was passed for effecting certain improvements in the University of Oxford. Under that act a commission was appointed, consisting of men of learning and high station, to advise and cooperate with the governing body, and so effect such changes as might be useful. At present it can scarcely be said that science at Oxford receives any substantial encouragement. The fellowships are for the most part close, and therefore are not necessarily the rewards of learning; and where they are open, the success of the candidate depends upon his proficiency in the ancient languages and literature. Honours are indeed awarded to the mathematical and physical sciences, but they carry with them no emoluments; and without any knowledge of the mathematical sciences except the elements of plane geometry, and without any knowledge whatever of the physical sciences, the highest University honours may be obtained. A man therefore, after having very creditably passed a public school, and having taken his degree with a first class *in literis humanioribus*, may find that he knows no more than was known 1800 years ago. He may be ignorant of physics in its most elementary form, and may therefore be incapable of comprehending the first principles of machinery and manufactures, or of forming a just and enlarged conception of the resources of this great country. That the legislation of last session should long continue unfruitful, I think, is improbable, and the time seems to be at hand when the cultivation of the physical sciences will receive a new impulse at our universities, and when the great resources of Oxford will, in part, be applied as the rewards of scientific eminence.

You are all, Gentlemen, no doubt aware, that in 1823 your Council, at the request of the Lords of the Treasury, appointed a Committee to report upon Mr. Babbage's plan for the construction of a Calculating Machine, which he called a Difference Engine. The Committee, I need hardly say, was composed of men eminent for their theoretical and practical acquaintance with such subjects: that Committee recommended the Lords of the Treasury to assist Mr. Babbage in carrying out his undertaking. The Lords of the

Treasury acquiesced, and the work was proceeded with; Mr. Babbage exercising a constant and vigilant superintendence, furnishing the designs, making the computations, in fact supplying all the theoretical requirements, while the Government supplied the manual labour and raw materials. In the then backward state of mechanical engineering, great difficulties were encountered; at length in 1828 the Royal Society was again consulted by Government, and the result was a report from a Committee, to the effect that satisfactory progress had been made, considering the difficulties, and that the engine was likely to answer the expectations of its inventor. The Council adopted the report, and communicated it to Government, with a strong recommendation in favour of the undertaking. The Government acting under that recommendation supplied further funds, on the condition that the engine was to be public property, and the work proceeded. In 1830 the Royal Society was again consulted by Government, and the Council acting as on former occasions appointed a Committee. The report which was drawn up in a detailed form was satisfactory to the Treasury, and the Council were informed that funds would be supplied from time to time till the engine was completed. Very soon a new difficulty occurred; it became necessary to change the engineer, and it was then found that by the rules of the trade, the tools, which had been constructed at the public expense, were the private property of the engineer: there was no choice, therefore, but to sacrifice the tools, or to endeavour to effect a compromise for a large sum. The progress of the work was suspended: there was a change of government. Science was weighed against gold by a new standard, and it was resolved to proceed no further. No enterprise could have had its beginning under more auspicious circumstances: the Government had taken the initiative, they had called for advice, and the adviser was the highest scientific authority in this country;—your Council, guided by such men as Davy, Wollaston and Herschel. By your Council the undertaking was inaugurated, by your Council it was watched over in its progress. That the first great effort to employ the powers of calculating mechanism, in aid of the human intellect, should have been suffered in this great country to expire fruitless, because there was no tangible evidence of immediate profit, as a British subject I deeply regret, and as a Fellow my regret is accompanied with feelings

of bitter disappointment. Where a question has once been disposed of, succeeding Governments rarely reopen it; still I thought I should not be doing my duty if I did not take some opportunity of bringing the facts once more before Government. Circumstances had changed, mechanical engineering had made much progress, the tools required and trained workmen were to be found in the workshops of the leading mechanists, the founder's art was so advanced that casting had been substituted for cutting in making the change wheels even of screw-cutting engines, and therefore it was very probable that persons would be found willing to undertake to complete the Difference Engine for a specific sum.

That finished, the question would then have arisen, how far it was advisable to endeavour, by the same means, to turn to account the great labour which had been expended under the guidance of inventive powers the most original, controlled by mathematics of a very high order; and which had been wholly devoted for so many years to the great task of carrying the powers of calculating machinery to its utmost limits. Before I took any step, I wrote to several very eminent men of science, inquiring whether in their opinion any great scientific object would be gained, if Mr. Babbage's views, as explained in Ménabrèa's little essay, were completely realized. The answers I received were strongly in the affirmative. As it was necessary the subject should be laid before Government in a form as practical as possible, I wrote to one of our most eminent mechanical engineers to inquire whether I should be safe in stating to Government that the expense of the Calculating Engine had been more than repaid in the improvements in mechanism directly referable to it: he replied, unquestionably. Fortified by these opinions I submitted this proposition to Government:—that they should call upon the President of the Society of Civil Engineers to report whether it would be practicable to make a contract for the completion of Mr. Babbage's Difference Engine, and if so, for what sum. This was in 1852, during the short administration of Lord Derby, and it led to no result. The time was unfortunate, a great political contest was impending, and before there was a lull in politics, so that the voice of Science could be heard, Lord Derby's government was at an end.

Although, in communicating with Lord Derby, I was not acting under the directions of your Council, still, as my object was to induce the Government to complete a work in which this Society had taken so great an interest, I conceived it to be my duty to lay the facts before you, as a basis to proceed upon, should it hereafter be considered expedient to renew the subject.

I have detailed to you regularly at each anniversary the proceedings of the Committee of Recommendations; a Committee, as you are all probably aware, appointed to distribute the grant for scientific objects which was made to us by Government the first year of my Presidency, and has since been continued annually. On the present occasion I have only to say, that since the last anniversary numerous reports have been received, and I hope the new Council will consider it expedient to collect the facts brought out, and arrange them in the form of a paper to be laid before Parliament.

With respect to the design for the re-examination of the heavens in the southern hemisphere, originally suggested by the British Association, and subsequently matured by your Council, I have only to say, as I said the last anniversary, that it is in the hands of Government.

There is no other subject which seems to me to call for observation; the report of your Treasurer will give all necessary information as to financial matters, and it only remains for me to express the deep sense I feel of the great services which have been rendered to Science by your Councils during the six years I have been officially connected with them. I am sure nothing could have exceeded their pains-taking industry, their complete devotion to your service. In their hands your interests were watched over with anxious care, they were in perfect safe-keeping; and when I was unavoidably absent, as was too often the case, I had no misgivings. To your Council I return my sincere thanks; to you, Gentlemen, I feel equally grateful; and in retiring from your Presidency permit me to assure you, that although the position I am destined henceforth to occupy will be less prominent, my exertions for the welfare of your Society shall not be less earnest.

Dr. Sharpey,

I am happy to have the honour of handing to you the Copley Medal, in charge for Professor Müller.

The Copley Medal has been awarded to our distinguished Foreign Member, Professor Johannes Müller of Berlin, for his important contributions to different branches of Physiology and Comparative Anatomy, and particularly for his researches on the Embryology and Structure of the Echinodermata, contained in a series of memoirs published in the Transactions of the Royal Academy of Berlin.

No one has borne a more conspicuous part in the advancement of physiological science for the last quarter of a century than Johannes Müller, and there is none whose services in that department of natural knowledge are more deserving of honourable recognition.

So great, indeed, has been his scientific activity, that in the brief reference to his writings suited to this occasion, I am constrained to pass by much that is excellent and confine myself to those which most strikingly evince his merit in the several departments in which he has laboured.

At an early period of his career he published his well-known treatise on the Secreting Glands. In this work he traces the intimate structure of these organs in the varied conditions which it presents, from the lowest animals to man; and in particular he establishes on a more broad and satisfactory basis the true doctrine of the relation of the blood-vessels and ducts, as first correctly conceived by Malpighi; indeed, since the time of the great anatomist of Bologna, no general work had appeared on the subject to be compared with that of Professor Müller.

Among his numerous contributions to Comparative Anatomy, I may specially single out his series of memoirs on the Myxinoid Fishes. Of the scope and importance of this great work but a faint idea is conveyed by the title; for while the anatomy of a particular family of fishes may be said to form its text, there is an ample commentary, rich in new and original matter, in which the structure is compared in other tribes, and the facts sagaciously applied to the elucidation of great questions in animal morphology.

In Physiology, Professor Müller has proved himself equally a master. His "Handbook" has long held a high place in physiological literature, and under this modest designation not only presents

clear expositions and enlightened discussions of truths already known, but is enriched throughout with the fruits of the author's own observation and experimental inquiry. Evidence of this may be found in almost every chapter, but it is sufficient to refer to his examination of the blood, to his disquisitions on the nervous system, and especially to his valuable experimental investigations on the voice and on hearing.

Professor Müller early applied himself to the study of the structure and economy of the Echinoderms. After describing, in a special memoir, the anatomy of the Pentacrinus, so interesting as a living representative of the extinct Crinoidea, and publishing, in conjunction with M. Troschel, a systematic arrangement and description of the Asterida, he was at length happily led to investigate the embryo life of this remarkable class of animals. The field of inquiry on which he entered had scarcely been trenched upon before, and he has since made it almost wholly his own by persevering researches carried on at the proper seasons for the last nine years, on the shores of the North Sea, the Mediterranean and the Adriatic. In this way he has investigated the larval conditions of four out of the five orders of true Echinoderms, and has successfully sought out and determined the common plan followed in their development, amidst remarkable and unlooked-for deviations in the larval organization and habits of genera even of the same order; and his inquiries respecting these animals have made us acquainted with larval forms, with relations between the larva and future being, and with modes of existence, such as nature has not yet been found to present in any other part of the animal kingdom. Finally, with the light thus derived from the study of their development, Professor Müller has subjected the organization of the entire class of Echinoderms, both recent and fossil, to a thorough revision, and has added much that is new, as well as cleared up much that was obscure, in regard to their economy, structure and homologies. It is to these researches, which occupy seven memoirs in the Transactions of the Royal Academy of Sciences of Berlin, that more special reference is made in the award of the Medal. Besides their other claims to distinction, they may be justly regarded as revealing a new order of facts in the history of animal development.

Dr. Hooker,

It was fortunate for natural science that you succeeded in obtaining the appointment of Naturalist to the Antarctic Expedition under Sir J. C. Ross, and that you fully availed yourself of the opportunities of pursuing your favourite studies which so frequently presented themselves during the progress of that arduous voyage.

The collections made during this voyage were extremely important, and have served as the foundation of a series of works illustrative of the botany of the Southern hemisphere. The "Flora Antarctica," which was commenced immediately after your return, at once established for you a high place as a philosophical botanist, by the accuracy and completeness with which each subject is treated, as well as by the importance of the physiological questions there discussed. The value of the details of a systematic work can be best appreciated by those who use it as a guide, but the essay on the structure and affinities of the curious parasite Myzodendron, may be noticed as perhaps the most striking of the many special topics which are there treated of.

The peculiar configuration of the Southern hemisphere, in which the land bears so small a proportion to the sea, seems at an early period to have directed your attention to Geographical Botany, and to have led you to investigate critically one of the most difficult questions of natural science, which is now acquiring that prominence to which it is so well entitled,—I mean the question of the origin and distribution of species. In your memoirs on the vegetation of the Galapagos Islands, you have brought together a great number of facts relative to insular floras, which throw much light upon this point of abstract science; and in your Flora of New Zealand (now in progress), you have discussed the question in all its bearings, in an essay which has attracted much attention, from the cautious and philosophical manner in which the subject is treated.

As Botanist to the Geological Survey of Great Britain, your attention was directed to the investigation of the extinct Flora; and it is evident from the essay on the carboniferous vegetation published in their Records, that you devoted yourself to this task with the same energy which had characterized your previous labours. In this essay an intimate knowledge of recent structure is applied to throw light upon the vegetation of remote periods in the history

of the globe, and there is evinced a just appreciation of facts and a cautious spirit of induction, which make it one of the most important contributions ever made by Botany to Geology.

In the selection of the Himalaya as the field of further exploration, you seem to have been guided by a sagacious perception of the requirements of natural science; and in the plain and artless narrative of that journey, we know not whether most to admire, the industry by which alone so much could have been done, the judicious selection of subjects of investigation, or the completeness of the results. On that work, geographers, geologists, meteorologists, and botanists, in fact, cultivators of all branchesof natural science, have pronounced a unanimous verdict, which may be best summed up in the words of the illustrious Humboldt, the greatest of scientific travellers, as "a perfect treasure of important observations, in which a prodigious extent of previous knowledge is brought to bear upon every topic, and which is marked with great sagacity and moderation in all the views brought forward."

Dr. Hofmann,

A Royal Medal has been awarded to you, "for your Memoirs on the molecular constitution of the Organic Bases, contained in the Philosophical Transactions, and the Transactions of the Chemical Society."

The long series of researches which are acknowledged in the present award, were commenced by your inquiries, published in 1844, on the volatile bases contained in coal-gas naphtha, in which you recognized aniline, a base previously obtained from indigo, and leucoline, likewise derived from the decomposition of quinine, cinchonine, and strychnine. In consequence of its extremely definite character, aniline was selected by you as the type of volatile bases, and investigated in all directions, with singular perseverance and success. A variety of new compounds were thus obtained, bearing a fixed relation to the primitive body, such as chloraniline, bromaniline, nitraniline, melaniline, &c. From these researches, and the facts supplied by other investigators, you were gradually led to a conception of the common constitution of this class of compounds, and obtained the means of producing substances of a similar constitution to an almost unlimited extent. Oxide of am-

monium is the general prototype, the hydrogen of that volatile base being replaced to the extent of one, two, three, or four equivalents, by a multitude of elementary and compound radicals. Your method, also, of introducing these radicals into the constitution of ammonia, by the agency of the bromides and iodides of the radicals, has been found to admit of extensive application, and has very materially assisted in the general progress which organic chemistry has made since this method was made public.

Dr. Neil Arnott,

I have much pleasure in announcing, that a Rumford Medal has been awarded to you, " for your construction of a new Smoke-consuming and Fuel-saving Fire-place, with accessories, ensuring the healthful warming and ventilation of houses, lately described in the Journal of the Society of Arts (May 12, 1854), and for your various contributions to the elucidation of the principles and improvement of the practice of heating and ventilation."

The President then called upon Dr. Sharpey to read the following obituary notices of some of the deceased Fellows :—

Edward Forbes.—In the melancholy list of those who have passed away from among the Fellows of this Society, there is no name whose mention will awaken a more general and profound feeling of regret than that of Edward Forbes. Some leave us full of years and honours, their work in this world finished, and its rewards enjoyed; the sphere of action of others has been so limited, that their absence is felt within only a narrow circle; but in Edward Forbes we have to lament one whose vigorous intellect had but just attained the ripeness of the prime of life; who, after rising with almost unexampled rapidity to the height of his ambition, sank within sight of a future more brilliant than his past; and whose loss to the brethren in science who looked up to him, to the University whose hopes were centred in him, and to the friends of all classes and pursuits who loved him, is truly irreparable.

A native of the Isle of Man, and born in the year 1815, Edward Forbes exhibited at a very early age that aptness for and attach-

ment to intellectual pursuits, so commonly remarked in those who attain to scientific eminence. Before his twelfth year, we find the boy already collecting and cataloguing the curious and beautiful things scattered along the shores of his native island, and even boldly venturing, without other guidance or encouragement than such as were afforded by the vigour and hopefulness of a fresh and youthful mind, amidst the mazes and difficulties of a science then in its infancy,—that Geology to whose advance it was his destiny in after life so essentially to contribute. And the fine healthy audacity with which, in these childish days, he undertook to compile a Manual of British Natural History in *all* its branches, and carried out his project, according to his means and powers, is worthy of note, and might have led a judge of human nature to prophecy well of his future.

The complexion of this future, however, was for a time doubtful. The tendencies of Edward Forbes's mind were always as strong towards art as towards science; and, in very early life, the former appear to have been the stronger, for we find him taking up his residence in London as an Art-student, under the guidance of the late Mr. Sasse. These labours in the studio were not of very long duration; but, short as they were, the development which they gave to a naturally great power of drawing, and the critical eye for form which they conferred, proved of essential importance to the future Naturalist and Professor. Again, the readiness with which Forbes's rich and overflowing humour embodied itself in sketches, vignettes, and caricatures,—a facility which lent no small charm to many of his published works, and has left many a pleasant memorial among his friends—must be regarded as not a little due to this early training.

However, the scientific tendency of Edward Forbes's mind appears to have been too strong to allow of any lasting or exclusive attachment to other pursuits; and in 1830 he left London and Art, to commence, as a student of medicine, the curriculum of the University of Edinburgh. It is hardly probable that he ever seriously looked forward to the practice of physic as a profession; for, although a diligent attendant upon the prescribed courses, he never presented himself for his degree. But even if it were so, his inborn genius, fostered by the teachings of a Jameson and a Graham, soon diverted

his attention from the hospital and the operating theatre to the museum and the field, and led him to the conviction that the pursuit of knowledge for its own sake was the only satisfactory sphere for his activity, and that in devotion to science lay the vocation of his life.

An exploration of Norway, and the publication of a fauna of his native shores, were among the first fruits of Edward Forbes's scientific training. These were succeeded by excursions into Algeria and Illyria, and by a stay of some months in Paris, where Prévost was teaching geology, and Geoffroy and De Blainville zoology. In 1840, Forbes published the first work by which he will be remembered, 'The History of British Starfishes,' one of a series of monographs upon the natural history of this country which do honour to its zoologists. Although consisting of little more than a description of a score species of Echinoderms, this is, in many respects, an important and remarkable Essay; and it must be considered to be by no means its least merit, that, with an extent and thoroughness of knowledge rarely exceeded, it unites a spirit of playful and elegant humour, rare in itself, and still more rare in such combination. Repudiating that stilted and pretentious solemnity sometimes thought essential to the due preservation of the dignity of science, Edward Forbes here exemplifies the doctrine upon which his whole life was a commentary,—that a true philosopher must first, and before all things, be a genial and simple-minded man.

Mr. Forbes spent the succeeding two years as Naturalist in H.M.S. Beacon, then commanded by Capt. Graves, the chief of the surveying corps at that time employed in the Mediterranean. At this time Capt. Graves was more particularly occupied with the Ægean Sea, and for a short period he was engaged in affording assistance to Sir Charles Fellowes's expedition in Lycia. Availing himself of the opportunity thus afforded, Mr. Forbes, in company with Lieutenant (now Captain) Spratt and the Rev. Mr. Daniell, made many excursions inland. The terrible fevers of the Levant did not spare the travellers; and while one of his companions fell a victim to their virulence, Forbes's own life was at one time despaired of, and he always considered his constitution to have been permanently injured by the attack.

The results of these combined explorations, by which the forgotten

sites of many ancient cities were determined, were eventually published by Messrs. Spratt and Forbes; but these 'Lycian Travels,' however interesting, must be considered as of very secondary importance, so far as Edward Forbes is concerned, to the results of his examination of the shores of the Ægean with the dredge, an instrument of great simplicity indeed, but with whose value he had early acquainted himself. It was upon the data obtained by dredging during his cruises that he based that remarkable 'Theory of the Bathymetrical Distribution of Life' with which his name will always stand most prominently connected in the annals of science.

That zones of distinct species of living beings may be shown to exist at different depths in the sea—just as corresponding zones are demonstrable at different heights on the land,—is a proposition which had been clearly enunciated by Audouin and Milne-Edwards so long ago as 1832, and subsequently, on independent grounds, by Lovèn. But, for the addition of new zones, for their accurate enumeration and definition, and above all, for the practical application of the 'Theory of Distribution in Depth, of Marine Life,' to the solution of geological problems, we are entirely indebted to Edward Forbes.

It is impossible to estimate too highly the value of the 'Theory of Bathymetrical Distribution' as a contribution to scientific natural history; and it is greatly to be regretted that the details of the Ægean researches, from which the theory was constructed, have never been published. A sum of money was granted by Government for the purpose, but the pressure of new avocations and other practical difficulties interfered. Nevertheless, Mr. Forbes always looked forward to the working up and publication of these his early and favourite investigations; and doubtless, the hoped-for leisure to carry out his palns was one of the many attractions which the Chair of Natural History in Edinburgh offered. But these projects were destined to have no fulfilment; and Forbes's views have but an incomplete representation in the 'Reports of the British Association' and the 'Memoirs of the Geological Survey of Great Britain.'

Edward Forbes's studies at the University were on the widest scale; and like the father of Natural History, he attacked with equal ardour Mineralogy, Botany, and Zoölogy. Good authorities affirm that his knowledge of mineralogy was of no small extent; as a

zoologist, all know his merits; and as a botanist, his acquirements were so well thought of, that during his absence in the East, the Chair of Botany at King's College, London, vacated by the death of the lamented Professor Don, was, without solicitation on his part, conferred upon him. The intelligence of this appointment put an end to his meditated project of further travel in Egypt and on the shores of the Red Sea, and Professor Forbes returned to enter upon the duties of his post in 1843.

Launched into the great world of London, Forbes's further progress was not more due to his intellectual than to his moral characteristics. His singular sociality and geniality, the gentleness of his manner, and his ready sympathy with and comprehension of all phases of human character, won for him a prominent place, whatever the society into which he was thrown. Among his fellow-students, Edward Forbes was the leader,—whether the thing to be done were the editing of a mock 'Maga,' the writing and illustrating a song, or a downright piece of hard work. He had no quarrels himself, soothed them among others, and altogether kept men together as no one else could do.

It was these rare peculiarities which, together with his high intellectual qualifications, recommended him to the authorities of the Geological Society, when a vacancy occurred in the Curatorship of their Museum.

Professor Forbes accepted this post in 1842, and availing himself of the opportunities for the study of palæontology thus afforded him, he soon distinguished himself in the field to which henceforth his energies were principally directed. In fact, when in 1844 he resigned his Curatorship, it was to join the Geological Survey of Great Britain, under the direction of Sir Henry De la Beche, and to take the official position of Palæontologist to the Survey, a post in discharging whose duties he spent the next ten years of his life.

When the Museum of Practical Geology and the Government School of Mines were established—growing as they did out of the Survey,—Prof. Forbes directed the arrangement of the beautiful collection of fossils now displayed in the former, and became Lecturer on Natural History and Palæontology in the latter part of the Institution in Jermyn Street. At the same time, many valuable contributions to the 'Transactions of the Geological Society,' to the

memoirs published by the officers of the Survey, to the beautifully illustrated 'Decades,' in which Prof. Forbes's artistic skill and judgment are so manifest; his Monograph on the British Naked-eyed Medusæ, published by the Ray Society; and the large work on the 'Natural History of the British Mollusca,' undertaken and finished in conjunction with Mr. Hanley—are sufficient evidence that his active mind was not one of those which are oppressed and overpowered by details.

Among these many contributions to science there are two so marked by originality and genius, so pregnant with results for the future, as to deserve more than passing notice. The first of these is the 'Essay on the Distribution of the Fauna and Flora of the British Isles,' in the first volume of the Memoirs of the Survey, which may be characterized as one of the happiest applications of the facts of one science to the elucidation of the difficulties of another that has ever been made. The doctrine laid down in this memoir is, that the existing distribution of animals and plants can only be regarded, not as a primary and independent phenomenon, but as the result of previously existing conditions,—as the product, in fact, of two factors; the one, the successive changes of living beings in time; the other, the successive changes of the position and boundaries of land and sea in space.

The second work here adverted to is that remarkable Essay on the Tertiary Beds of the Isle of Wight, the fruit of Prof. Forbes's last labours.

By an elaborate study of the Purbeck beds in Dorsetshire in 1850, Prof. Forbes had come to the unexpected conclusion that they were divisible into three formations, each characterized by distinct sets of fossil remains; and that the freshwater mollusca and foraminifera of the Purbeck beds, to which he had given his more particular attention, did not agree specifically with the fossils of the incumbent Hastings Sands. The latter he proposed to class as Lower Cretaceous or Neocomian, while the Purbeck he henceforth considered as an uppermost member of the Oolitic group. His great success in these researches awakened in him a lively curiosity to examine in like minute detail the great series of tertiary freshwater strata occurring in the northern part of the Isle of Wight. Accordingly, he devoted several months in the autumn and winter of 1852 to the

exploration of that complicated succession of freshwater and fluvio-marine beds; and when we reflect how many able geologists had gone before him in this field, we may well marvel at the richness of the harvest which he reaped. Among other novel conclusions, he showed that certain strata called the Headon beds in Alum Bay had hitherto been incorrectly identified in age with the Bembridge limestone of Whitecliff Bay at the opposite or eastern end of the island. These Bembridge beds, which belong to the same division as the well-known calcareous building-stone of Binstead, were recognized as the true equivalents in age of the celebrated gypseous series of Montmartre near Paris, containing similar remains of Paleotheria and other extinct quadrupeds, which Cuvier had long before described. It followed from the correction here alluded to, that the mammiferous fauna of Binstead held a much higher place in the Eocene series than the Headon beds, and, consequently, than the contemporaneous Hordwell strata of Hampshire, in which other quadrupeds than those of Binstead, including amongst them the Paloplotherium of Owen, had been detected. Between these two divisions, called by Forbes the Bembridge and the Headon, he found another, which he called the Osborne or St. Helen's series, also of fresh and brackish-water origin, and distinguished by peculiar species of mollusca.

In addition to all these results, Prof. Forbes brought to light an entirely new member of the British tertiary series, hitherto overlooked. Near Yarmouth, in the Isle of Wight, the most elevated ground formed by tertiary deposits is called Hempstead Hill, in which strata corresponding in their fossils with the Limburg beds of Belgium or the Grès de Fontainebleau in France, were recognized. These would be classed by many geologists of the continent as Lower Miocene; but Prof. Forbes inferred, from the gradual passage which he traced between them and the subjacent Bembridge series, that the whole should rather be regarded as Upper Eocene. In a word, he declared it to be impossible, without drawing an arbitrary line of demarcation, to denominate the Bembridge beds Eocene, according to received usage, and to distinguish the Hempstead strata as Miocene.

Always an active and influential member of the Geological Society, Prof. Forbes became its President in 1853. His anniversary address

for that year contains a sketch of a remarkable theory with regard to the relation of the past and present faunas of the world to one another,—the Theory of the Polar Development of Life in Time.

Prof. Forbes's occupation of the Presidential Chair in the Geological Society, however, was but short; for in the spring of 1854, the death of Prof. Jameson placed within his reach the ambition of his life,—the Chair of Natural History in the University which had been his *alma mater*. Appointed and called upon to enter at once on the duties of this distinguished office, he commenced with what an eminent fellow-worker has well called "the light-hearted intensity peculiar to himself," to conceive and to inaugurate plans of a magnitude proportioned to his great powers and noble aspirations. But a slow, though mortal disease—suspected least of all by himself—had long been undermining his constitution; and its sudden outbreak, accelerated by over-fatigue and cold, carried him off, after a very short illness, on the 20th of November, 1854. He was buried at Edinburgh, with such great public demonstrations of respect as have been rarely shown, but which after all but faintly represented the profound and universal sorrow.

Rear-Admiral Sir John Franklin, K.C.H., D.C.L. etc.—Although the unauthenticated intelligence brought recently to England by Dr. Rae respecting Sir John Franklin and his companions does not raise the veil of mystery which shrouds their fate, yet the touching relics of that gallant commander and his brother-officers are unhappily of a nature, not only to awaken the most gloomy thoughts, but to forbid us entertaining any longer the cherished hope that they may be restored to their country.

At an early period of this year, long before the Expeditions which were sent to search for the 'Erebus' and 'Terror' could have returned, and of course prior to the receipt of the recent Esquimaux report, the Admiralty removed the names of Sir John Franklin, his brother-officers and crew from the Navy List. This official act, and the recent melancholy tidings bearing upon their fate, have rendered it necessary to include in the list of deceased Fellows the names of Sir John Franklin and Captain Crozier, both of whom there is too much reason to apprehend have perished in their heroic endeavours to bring to a successful issue the great enterprise confided to them.

Sir John Franklin was descended from a respectable Lincolnshire family, who occupied a small estate for several years in that county. In consequence of the improvidence of his grandfather, Sir John's father was obliged to enter into business, in which he was so successful as to have been enabled to give his sons a good education. One, Sir Willingham Franklin, rose to the rank of a Chief Justice of Madras; another, Major Franklin, of the Bengal Cavalry, was distinguished for his scientific geographical knowledge, which obtained for him the Fellowship of this Society.

The fourth son, the subject of this notice, who was born at Spilsby in Lincolnshire in 1786, was intended for the Church, but while still at school, he took advantage of a holiday to run from Louth to the coast for the purpose of seeing the ocean, on which it is stated he gazed for hours with wonder and delight. His father, who was extremely desirous that his son should not follow the profession of a sailor, for which he manifested the strongest partiality, conceived that by sending the boy in a small merchant-ship to Lisbon, the discomforts of the voyage would effectually cure him of his love for the sea; but in the case of young Franklin, as in many others, this expedient had a totally different effect, so that being evidently bent on a maritime career, he was entered as midshipman on board the 'Polyphemus' in 1800, and was in that ship at the battle of Copenhagen. On this occasion he escaped without a wound, while a brother midshipman was killed at his side.

He next sailed with his maternal cousin Captain Flinders on his celebrated voyage of discovery to Australia, during which he acquired most of that skill and knowledge which was of infinite value to him in after-life.

In the course of this survey he had the misfortune to be wrecked with his commander on a coral reef in August 1803, and by his conduct during seasons of great hardship gained the praise and esteem of his superior officers.

Franklin next acted under Captain Dance in the 'Earl Camden,' and had charge of the signals during the celebrated engagement in the Straits of Malacca, with the French Admiral Linois.

On his return to England he was appointed to the 'Bellerophon,' Captain Loring, in which ship he had the honour of acting as signal midshipman in the memorable battle of Trafalgar; a post of great

danger, as the 'Bellerophon' was engaged yard-arm to yard-arm with the 'Aigle,' a French seventy-four, and during the action the poop where he was stationed was repeatedly swept by the enemy's musquetry. Out of forty companions, only six, beside himself, escaped without wounds or death.

During the subsequent two years, Franklin, who was now mate, served in the Channel Fleet and the Rochefort Squadron, under Admiral Cornwallis, Lord St. Vincent, and Sir Richard Strachan.

He then joined the 'Bedford,' one of the convoy which escorted the Emperor of Brazils to South America. Immediately afterwards, he sailed in the Expedition against New Orleans, where he distinguished himself particularly in the attack on the American gun-boats, on which occasion he was wounded. His heroic conduct in this gallant affair was prominently mentioned in the official despatches, and led to his being promoted to a Lieutenancy in the 'Forth,' which ship conveyed the Duchess d'Angoulême to France on the restoration of the Bourbons.

In 1818 commenced the brilliant series of Arctic Expeditions with which Franklin's name is so honourably associated. From the moment of their having been projected he evinced the strongest desire to be engaged in them, and he was indebted to Sir Joseph Banks, at that time President of the Royal Society, for the gratification of his wishes.

It has been stated, that, with the view of proving himself qualified for surveying operations, Franklin surveyed a portion of the City of London by triangulations taken from church steeples and towers, and that he was in a great measure indebted to the successful result of this undertaking for Sir Joseph Banks's patronage and support.

Sir Joseph, who had considerable influence with the Admiralty in all matters relating to Arctic exploration, strongly recommended his young friend for Arctic service, and he was accordingly appointed to the command of the 'Trent.' This ship, with the 'Dorothea,' formed an expedition under the command of Captain Buchan, the object of which was to attain the North Pole, and to enter the Pacific through Behring's Strait. The ship sailed in the early part of 1818, and reached the latitude of 80° 34′ North, when the 'Dorothea' became disabled by severe pressure from the ice, and was incapable of proceeding further. But, although dangers of the most appalling

nature were around Lieut. Franklin, yet as his ship was less damaged than that of Captain Buchan, he earnestly requested permission to proceed alone in the execution of this discovery. The nature of Captain Buchan's instructions prevented this, and the Expedition returned.

Immediately on his return he was appointed to the command of that celebrated Expedition to explore the North American coast, which occupied the years 1819, 1820, 1821, and 1822, the history of which, as told in his own manly and unaffected language, is undoubtedly one of the noblest pictures of heroic exertion and patient endurance.

The results of the labours of Franklin and of his distinguished associate Sir John Richardson, in this memorable journey, deserve more full and fitting recognition than can be attempted on this occasion; suffice it here to observe, that a vast extent of the North American Continent, before unknown, was added to our Charts, and large acquisitions gained for science by the careful study of the physical geography and natural productions of that portion of the globe.

Undeterred by the appalling sufferings he had already undergone, Franklin, although lately united in marriage to Miss Porden, again volunteered his services for Arctic exploration. These were accepted, and in the course of 1825–27 an additional tract of the North American Continent was carefully surveyed.

For these arduous services, which extended over a period of twelve years, and in the execution of which he travelled nearly 9000 miles, and added a coast-line of upwards of 1200 miles to our North American Maps, he was promoted to the rank of Captain, knighted by his Sovereign, and had the degree of D.C.L. conferred on him by the University of Oxford. He also received the Gold Medal from the French Geographical Society, and was elected a Fellow of the Royal Society, on whose Council he served in 1829 and 1830.

In the former year, having had the misfortune to lose his first wife, he married the present Lady Franklin, then Miss Jane Griffin, whose persevering devotion in endeavouring to rescue her unfortunate husband is well known.

He now remained at home for two years, when he was appointed to the 'Rainbow,' and served in that ship in the Mediterranean for

three years. He was chiefly employed in the Greek waters, and had the good fortune to be of considerable service in the delicate adjustment of complicated diplomatic relations.

During this period, as indeed on all other occasions, he eagerly availed himself of every opportunity, not only to improve his knowledge of geology, to which science he was greatly attached, but also used every exertion to add to the museum of the Geological Society and to the private collections of scientific men.

After a brief period of rest, which followed his services in the Mediterranean, he applied to Lord Glenelg for employment under the Colonial department, and his Lordship, in a very complimentary manner, offered him the important post of Governor of Van Diemen's Land, which he held for seven years.

During this time, that Colony received the convicts sentenced to transportation, New South Wales having ceased to be a penal settlement, which rendered Sir John Franklin's position most onerous and trying. But he acquitted himself so entirely to the satisfaction of the colonists, that in grateful remembrance of his government, which was marked by the establishment of a College and a Philosophical Society, they, unsolicited, subscribed £1600 towards the expenses of the recent private expedition fitted out for his rescue.

It might be supposed that after so long a period of laborious services, Sir John Franklin would have desired repose, particularly as he had now attained high renown; but his wishes still pointed towards active employment, and consequently, when the Arctic Expedition was contemplated, which in all human probability has cost him his life, he was willing and ready to take the command, when the Admiralty were of opinion that he was the officer best fitted to act as its chief.

That Expedition, as will be remembered, was originated by the late Sir John Barrow, who, before resigning his office of Secretary to the Admiralty, submitted a plan for the discovery of the North-west Passage to that branch of Her Majesty's Government, by whom it was referred to the Council of the Royal Society.

Without concurring in all Sir John Barrow's views, the Council gave it as their opinion that such an Expedition was likely materially to increase our knowledge of geography and terrestrial magnetism, and to promote the general interests of science, and that it was at

that time peculiarly desirable in connexion with the magnetical inquiries then in progress.

The history and fate of the Expedition, which left our shores in May 1845, are still veiled in obscurity; this, however, we know, that every thing was done to render it efficient,—that the officers under Sir John Franklin were men of experience and zeal, and that the last accounts which were received from them represent their commander as animated by all the ardour and spirit which characterized his early Arctic exertions.

It would have been unjust to have expected less from such a man, and as his instructions contained the usual discretionary latitude given in these documents, there is too much reason to fear that in his great anxiety and daring attempts to solve the problem of the North-west Passage, his ships became inextricably entangled in the thick-ribbed ice of the Arctic regions, and his attempts to reach the North American continent were rendered abortive.

But although the few facts that have reached us point to the dreary shores of the Arctic regions as the final resting-place of our lamented Fellow and his brave companions, his memory will ever be enshrined on British land within British hearts, as an explorer as eminent in discovery, as he was patient under trial and privation, and kind and good in all the relations of life.

Captain Francis Rawdon Moira Crozier, R.N., the companion of Sir John Franklin on his last and fatal voyage, and second in the command of the expedition, was eminently qualified for the post by long experience in the navigation of the icy seas.

He accompanied Sir W. E. Parry on his last three voyages to the Arctic Regions, and was made Commander for his services as First Lieutenant of H.M.S. 'Cove,' under Captain James Clark Ross, and despatched in the depth of winter to afford assistance to the missing whalers supposed to have been frozen in the pack of Davis Strait.

Again, as second in command to Captain J. C. Ross, he obtained post rank for the first season's successful operations of the expedition sent, at the recommendation of the Royal Society and British Association, for purposes of scientific research and geographical discovery to the Antarctic Ocean.

Captain Crozier was distinguished for devotion to his duties as an

officer, zeal for the advancement of science, and the untiring assiduity and exactness of his magnetic and other observations. The Transactions of the Royal Society, as well as the published results of the Antarctic Expedition, bear ample testimony to his diligence and ability.

Robert Jameson was born at Leith on the 11th of July 1774. Of his early years it is reported that he showed a decided bent towards the study of external nature, and although he went through the course of apprenticeship and college study then usual for young men entering the medical profession, he never engaged in practice, but devoted himself to the pursuit of natural history as the main occupation of his life.

The first fruits of his labours as an original inquirer were given to the world in his "Outlines of the Mineralogy of the Shetland Islands and of the Island of Arran," published while he was yet a very young man; and this was followed by a more important work entitled the "Mineralogy of the Scottish Isles," and containing the results of further local investigation. The success of these early essays served only as an inducement to extend and deepen the foundations of his knowledge, and with this view he spent nearly two years in the great school of mining and mineralogy at Freyburg, under the tuition of the celebrated Werner. Returning home from Germany, he was appointed in 1804 to the chair of Natural History in the University of Edinburgh, which had become vacant by the death of his early friend and preceptor, Dr. Walker. When thus established as a teacher in the chief seat of learning in his native country, he seems to have early entertained the project of a great work on the Geology of Scotland, in which the whole was to be described, county by county, and he made a commencement with "The Mineralogy of Dumfries." From this undertaking, however, he was soon called off to prepare various elementary and systematic works for the geological student; and accordingly a treatise "On the external Characters of Minerals," and a "System of Mineralogy and Geognosy" soon appeared from his pen, and, after a longer interval, his "Manual of Mineralogy and of Mountain Rocks." He also contributed several articles on different branches of Natural History to the Edinburgh Encyclopædia and

Encyclopædia Britannica, and to the Transactions of the Wernerian Society, of which he was the founder. In 1819, in conjunction with Dr., now Sir David Brewster, he commenced the Edinburgh Philosophical Journal, which was afterwards continued, under his sole editorship, under the title of the Edinburgh New Philosophical Journal, and this duty he continued steadfastly to perform to the end of his life.

Mr. Jameson's strong point was Mineralogy; and as he held a knowledge of minerals to be valuable chiefly as subservient to practical geology and mining, he paid less regard to chemical than to external characters in the definition and determination of mineral species. His consummate acquaintance with the mineral characters of rocks stood him in good stead in the great controversy of which Edinburgh became the centre, between the respective supporters of the Plutonian and Neptunian theories of the earth. Trained in the school of Werner, and deeply reverencing his great master, Mr. Jameson naturally imbibed his views; and though he eventually became convinced of their insufficiency, and candidly avowed his change of opinion, it is admitted that the doctrine which finally triumphed gained much in solidity and precision by the searching ordeal to which it had been subjected at the hands of its accomplished opponent.

His College lectures owed no attraction to language or delivery, but the solid instruction imparted secured the earnest attention of his audience. His success as a teacher, however, was greatly due to his field lectures and geological excursions with his pupils in the country round Edinburgh, so rich in visible illustrations of geological science. These practical outdoor instructions, conveyed as they were in perspicuous and impressive language, and followed up by easy and unrestrained colloquial explanation, became the means of infusing a love for the study in many of his youthful followers, and of sending forth active and well-prepared geological labourers to most parts of the world.

Mr. Jameson was a member of many learned societies both at home and abroad; he was elected a Fellow of the Royal Society in 1826. In private life he was much esteemed and could reckon many attached friends. His death took place on the 19th of April 1854.

The Society has lost another distinguished member in George Newport, who died in April last.

The earlier incidents of Mr. Newport's personal history, although simple in themselves, are well deserving of record, as important passages in the life of one who, through inborn love of knowledge, just confidence in his own powers, indomitable energy and rigorous self-denial, raised himself to eminence from a humble walk of life, in spite of the difficulties he had to encounter from the want of early training and other aids which a more advantageous social position supplies.

George Newport was born on the 4th July 1803, in the city of Canterbury, where his father was a wheelwright, and at that time in comfortable worldly circumstances. The son gave early indications of mental activity, showing, as soon as he could read, a great fondness for books, and also a taste for drawing. This latter taste, though never aided by external cultivation, abode by him through life, and proved of great use to him in his subsequent studies by enabling him to represent accurately, and at the moment, the subjects of his investigation.

At the usual age he was sent to a day-school in Canterbury, where he received the ordinary English education obtainable by boys in his station of life. When he had reached his 14th year he was removed from school, and, very much, it is said, against his will, was bound apprentice to his father. Although he soon became an expert workman in the lighter branches of the trade, he never got the better of his dislike to the occupation, and often avowed his purpose of abandoning it at the expiration of his apprenticeship.

Before this period arrived, however, his future prospects, humble as they were at best, became still more clouded. His father, it is said from no fault of his own, but from unavoidable circumstances, became involved in pecuniary difficulties, and the whole of his little property had to be sacrificed for the benefit of his creditors. Under this change of circumstances, the son, instead of seeking, as he had hoped to do, for some more intellectual employment, was compelled to continue at his father's trade, and by working hard for three or four days in the week, he earned sufficient to enable him to devote the remaining days to pursuits more congenial to his mind. These were chiefly—reading on general subjects both literary and

scientific, the study of antiquities, for which the locality afforded great opportunities, and, most of all, the pursuit of his earliest and latest love, the observation and investigation of insect life. He had been a collector of insects from his early boyhood, but it was during the two years of comparative leisure following the close of his apprenticeship (from about July 1824 to November 1826), that his entomological studies assumed consistency and form.

The Canterbury Philosophical and Literary Institution afforded means of instruction of which George Newport, at this time, took full advantage, having joined it as a member in 1825. Its library, its collections, both in natural history and antiquities, and its lectures, were to him the source of endless recreation and instruction,—advantages, which he was soon able to repay, in kind, by contributions both to its museum and theatre. In September 1825 he began an elementary course of lectures on mechanics, and gave other lectures on the same subject, at intervals, both during this and the subsequent session of 1826; and in October of the latter year he was appointed General Exhibitor of the Museum, with a small salary. During this and the following year he gave some general lectures on entomology, as well as numerous demonstrations on the same subject, from his own collections and the specimens in the museum; and his name stands on the books of the Society as a large donor of British insects collected and preserved by himself.

Mr. Newport gave much satisfaction in his management of the museum, and made numerous friends among its chief visitors, the young men of the city, several of whom, subsequently, gave a striking proof how highly they estimated his character and services. Among the members, and one of the occasional Honorary Lecturers of the Society during the time of Mr. Newport's curatorship, was a Surgeon then residing at Sandwich. The intimacy arising from position and from pursuits of a kindred character, led to a connexion which ended in Mr. Newport's leaving Canterbury and going to reside at Sandwich; Mr. Weekes agreeing to receive him as his apprentice without the payment of any premium, but also without any remuneration for services, even in the form of board and lodging. All that he had of present or prospective means to meet the exigencies of such a position, consisted of a small sum in hand set apart from his own scanty earnings, and the generous offer of such

contributions from his young friends in Canterbury as their very moderate resources could supply.

This period of Mr. Newport's life was marked by the greatest privations, and he often, in after years, expressed some surprise how he was able to bear up against them. Whatever his expenditure was (and it was marvellously small), it was still supplied by the same attached friends at Canterbury. It is pleasing to be able to add, that all these precious debts were carefully recorded by him, and gradually liquidated, in after years, as the means of doing so came slowly into his possession.

At the expiration of his apprenticeship, Mr. Newport went to London to prosecute his medical studies. Through the friendly intervention of a Physician, to whom he had been fortunate enough to gain an introduction, he obtained a nomination to University College (then the University of London), at which he was entered on the 16th of January, 1832; and on a representation of his peculiar circumstances being laid before the Professors, they all most readily gave him gratuitous access to their respective lectures. There, besides attending to the ordinary branches of professional instruction, he became a diligent pupil in the class of Comparative Anatomy, under Dr. Grant. After the usual course of study, he received his qualification for practice both from the Company of Apothecaries and the College of Surgeons; and in April 1835 obtained the appointment of House Surgeon to the Chichester Infirmary. This appointment, slight as it was, may be said to have terminated his struggles for existence, and placed him, for the first time, in a position of comparative ease, security, and comfort. He, however, did not long retain the office, having resigned it in the beginning of 1837.

On leaving Chichester in January 1837, Mr. Newport returned to London, entering soon afterwards into partnership with a young Surgeon who had been for some time established in practice. This partnership continued several years, but was not productive of satisfactory results, either in a social or pecuniary point of view. On its dissolution, Mr. Newport became more and more occupied with his scientific pursuits, relishing his professional avocations less and less, and becoming, in some measure, unfitted for them, so that for a good many years before his death, the whole of his professional

income was almost exclusively derived from a single family, and never exceeded thirty pounds per annum. So circumstanced, his means were necessarily extremely circumscribed; but his habits were, happily, of the most frugal kind, and the most scanty lodging served him for all purposes of accommodation and study. But on the 1st July, 1847, Her Majesty was graciously pleased to grant him a pension of £100 a year, in consideration of his merits as a laborious and disinterested cultivator of science; and from this time, feeling his mind more tranquil in his improved worldly circumstances, he continued his labours more steadily and vigorously than ever, up to the day when he was stricken with his fatal illness.

That illness may truly be said to have been brought on by Mr. Newport's zeal for science. It had been his practice for some years to devote a day or two in the spring season to a search for frogs and other aquatic animals in the marsh lands west of London, in order to secure a supply of specimens for his physiological investigations. In these excursions he commonly contracted a cold, and on the last occasion this assumed the form of severe bronchitis, and being followed by fever of a typhoid character, terminated his valuable life on the 7th of April.

Mr. Newport was a member of the Entomological Society nearly from the time of its foundation, and during the sessions of 1844 and 1845 served the office of President. In 1847, he became a Fellow of the Linnean Society: the date of his election into the Royal Society is March 26, 1846. At the time of his death he was a Member of the Council.

The following is a list of Mr. Newport's writings:—

On the Nervous System of the *Sphinx Ligustri*. Phil. Trans. 1832.

On the Nervous System of the Sphinx (part ii.) during the latter stages of its Pupa and Imago states. Phil. Trans. 1834.

On the Respiration of Insects. Phil. Trans. 1836.

On the Habits of the Wasp. Trans. of Entom. Soc. vol. i.

On the Temperature of Insects. Phil. Trans. 1837.

Observations on the Anatomy, Habits and Economy of *Athalia centifoliæ*. Essay for Saffron Walden Society's Agricultural Prize. 1838.

The article "INSECTA" in the Cyclopædia of Anatomy and Physiology. 1839.

On the Organs of Reproduction and the Development of the Myriapoda. First Series. Phil. Trans. 1841. (Bakerian Lecture.)

On some new Genera of the class Myriapoda. Proc. of Zool. Soc. 1842.

On the Habits of Gregarious Insects. Trans. of Entom. Soc. vol. iii.

On the Habits of *Megachile centuncularis*. Trans. Entom. Soc. vol. iv.

On the Nervous and Circulatory Systems in Myriapoda and Macrourous Arachnida. Phil. Trans. 1843.

On the Existence of Branchiæ in a perfect Neuropterous Insect (*Pteronarcys regalis*). Ann. and Mag. Nat. Hist. 1843.

On the Means by which the Honey-bee finds its way back to the Hive. Trans. of Entom. Soc. vol. iv.

Monograph on the class Myriapoda; and a new arrangement of the class Articulata. (First and second Memoirs.) Trans. of Linn. Soc. vol. xix.

On the Reproduction of lost parts in Myriapoda and Insecta. Phil. Trans. 1844.

The Annual Address to the Entomological Society from its President. 1844.

On the Natural History, Anatomy and Development of the Oil Beetle, *Meloë*. (First and second Memoirs.) Trans. of Linn. Soc. vol. xx.

A List of the species of Myriapoda (order Chilopoda) in the British Museum, with a synoptic description of forty-seven new species. Ann. and Mag. Nat. Hist. 1845.

A List of the species of Myriapoda (order Chilognatha) in the British Museum, with descriptions of a new genus and thirty-two new species. Ann. and Mag. Nat. Hist. 1845.

A second Address to the Entomological Society. 1845.

On the Aqueous Vapour expelled from Bee-hives. Trans. of Linn. Soc. vol. xx.

Note on the Generation of Aphides. Trans. of Linn. Soc. vol. xx.

On the Formation and Use of the Air-sacs and Dilated Tracheæ in Insects. Trans. of Linn. Soc. vol. xx.

On the Anatomy and Affinities of *Pteronarcys regalis*, with the habits and descriptions of some American Perlidæ. Trans. of Linn. Soc. vol. xx.

The Anatomy and Development of certain Chalcididæ and Ichneumonidæ; with descriptions of a new genus and species of Bee-Parasites. Parts 1, 2 & 3. Trans. of Linn. Soc. vol. xxi.

On the Natural History, Anatomy and Development of *Meloë*. (Third Memoir.) Trans. of Linn. Soc. vol. xxi.

On the Identification of a new genus (*Anthophorabia*) of Parasitic Insects. Ann. and Mag. Nat. Hist. 1849.

Further observations on the genus *Anthophorabia*. Trans. of Linn. Soc. vol. xxi.

Further observations on the Habits of *Monodontomerus*. With some account of a new Acarus (*Heteropus ventricosus*). Trans. of Linn. Soc. vol. xxi.

On the Ocelli in the genus *Anthophorabia*. Trans. of Linn. Soc. vol. xxi.

On the Reciprocal Relation of the Vital and Physical Forces. Ann. and Mag. Nat. Hist. 1850.

On the Impregnation of the Ovum in the Amphibia. (First Memoir.) Phil. Trans. 1851.

On the Impregnation of the Ovum in the Amphibia. And on the Direct Agency of the Spermatozoon. (Second Memoir.) Phil. Trans. 1853.

On the Impregnation of the Ovum in the Amphibia, and on the early stages of Development of the Embryo. (Third Memoir.) Selected and arranged from the Author's MSS. after his death, by G. V. Ellis, Esq. Phil. Trans. 1854.

These publications, numerous as they are, were all produced within a period of two-and-twenty years. His more important researches were for the most part communicated to the Royal or Linnean Society, and on two different occasions they received the award of the Royal Medal.

His earliest inquiries were directed to the structure and economy of insects and other articulated animals, and his name first became generally known in science by his admirable memoirs on the Anatomy of the Nervous System of the *Sphinx Ligustri*, and the changes which that system undergoes during the metamorphosis of the insect. Continuing to prosecute these researches in the Crustaceans and other allied invertebrata, he arrived at the conclusion, that in all the higher Articulata, the central part of the nervous system consists of two pairs of cords, the one gangliated, the other not, which, in accordance with the views of Sir Charles Bell, he conceived to minister respectively to sensation and motion.

In a subsequent research on the nervous system of the *Iulus*, he observed in the central cords, a set of fibres which connect together adjacent nerves on the same side of the body, and then extend with them to the surface of the animal. These he regarded as associating in function the lateral nerves of the corresponding side, independently of the brain, in conformity with the views which were at

that time gaining acceptance on the mechanism of reflex action. In the same communication he related a remarkable set of experiments, showing the correspondence in function between the central part of the nervous system of the invertebrata and that of vertebrated animals.

The development of the embryo of the invertebrata largely occupied Mr. Newport's attention, and among other more or less valuable results of his inquiries, he made out the remarkable process of growth of the young Myriapod, by the interpolation of successive new segments at one determinate and limited region of the body. The paper in which these observations were communicated was nominated as the Bakerian Lecture for the year 1841.

In the latter years of Mr. Newport's suddenly interrupted life, he was led to investigate with his usual zeal and industry the recondite process of the impregnation of the ovum. He chose the egg of the Frog as the subject of his experiments, and recorded the results in three papers communicated to the Society, the last of which, partly prepared at the time of his death, and afterwards completed from his written memoranda by his friend Professor Ellis, is inserted in the present volume of the Philosophical Transactions. In his inquiries into this question he endeavoured to determine the several conditions which affect fecundation, whether depending on the state of the parent animals and their generative products or on the influence of extrinsic circumstances; but the main result at which he arrived was the confirmation, by his observations on the Frog, of the view already adopted by some physiologists on other evidence, that in the process of fecundation the spermatozoids actually reach the interior of the ovum.

In Mr. Newport's studies of insects and other invertebrated animals, it was more to his taste to investigate structure, function, and habits, than to occupy himself with zoological description and arrangement; but that he could ably deal with the classification and natural-history relations of animals is shown in his admirable monograph on the Myriapoda, in the Transactions of the Linnean Society.

Mr. Newport was endowed with singular aptitude for the pursuit he had chosen. His well-known skill of hand in minute anatomical research, and his ingenuity in devising and dexterity in

performing experiments, gave him great practical advantages; and these qualities were combined with patience and accuracy in observation, and fidelity in recording what he saw, apart from what he thought. He had a nervous temperament, which was, as usual, associated with mental activity, and in Mr. Newport this was rendered effective by immoveable stedfastness of purpose and untiring power of sustained application.

Most faithful as an observer of nature, Mr. Newport was no less upright as a man. He was deservedly loved by those who knew him best, was most kind towards all who did him justice, and full of gratitude to those who had aided him in his early struggles.

By the death of Dr. Wallich this Society has lost a highly distinguished Fellow and Vice-President, and the science of Botany one of its most zealous cultivators and ardent promoters.

Dr. Nathaniel Wallich was born at Copenhagen, on the 28th January 1786. He was educated for the medical profession and studied Botany under Vatel the eminent professor, at that time in the University of Copenhagen. In 1807 he entered the service of the Danish East India Company, and was stationed at Serampore. There his love of botany attracted the attention of Dr. Roxburgh, the superintendent of the Botanic Garden at Calcutta. After the seizure of Serampore by the British, Dr. Wallich was placed on the staff of assistant surgeons in the Bengal army, and his services secured for the Botanic Garden, to the temporary charge of which he was nominated in 1815, and finally confirmed in the appointment shortly afterwards. Before he had been four years in India, Dr. Wallich's ardour in the pursuit of his favourite science led to the first of a series of attacks of fever that gradually undermined his constitution, and in 1812 he repaired to the Mauritius for the renovation of his health. There he diligently explored the botany of the island, and contributed immense collections of live plants to Calcutta, thus early proving his ability to employ to the best interests of science the munificent allowances which were shortly afterwards placed at his disposal. At the head of the noblest botanical gardens in the world, supplied with a large staff of collectors and artists, and with provision for travelling expenses on a most liberal scale, Dr. Wallich applied himself with indomitable zeal and

industry to the extension of the gardens, the investigation of the East India Company's vast and then rapidly increasing dominions, and the establishment of a correspondence and exchange of living and dried plants, upon a scale which for magnitude and efficiency has never been surpassed by any scientific establishment whatever. During the first ten years of his incumbency he performed five extensive journeys; he visited Nepaul, then a terra incognita, in 1820–1822, and on his return through the pestilential Tarai, at the foot of the Himalayas, caught a second severe fever that obliged him to go to sea immediately after his arrival at Calcutta. This opportunity he turned to the best account, and as soon as he could rise from his bed and superintend his staff, he commenced diligently investigating the botany of the Bay of Bengal, Penang, and the Straits of Malacca.

Within less than another year Dr. Wallich was personally exploring the kingdom of Oude and the provinces of Rohilcund and Kamaon, reporting on their forests and other vegetable products; and in 1826–1827, he accompanied the British embassy to Burmah, visited Ava, and after that the newly-acquired provinces of Tenasserim and Martaban. Throughout this period he employed collectors in eastern Bengal, and in other parts of India which he could not himself visit; and he communicated (in the name of the Hon. E. I. Company) the products of these labours with a lavish liberality to the botanists of Europe, not only in the form of collections, but of voluminous observations and drawings.

Repeated attacks of illness obliged Dr. Wallich to repair to England, where he arrived on furlough in 1828, and applied himself assiduously for four years to the publication of his great work "Plantæ Asiaticæ rariores," in three volumes folio, and the distribution of his enormous collections to the principal public and private museums in Europe. This distribution, of which a catalogue was lithographed by his own hand, constitutes the most valuable contribution of its kind ever made to botanists, and is of itself a sufficient monument of one man's devotion to science.

Dr. Wallich returned to India in 1832, when it soon became apparent that his constitution was completely undermined by incessant labour of both mind and body. For several years he conducted the garden correspondence with his wonted zeal and vigour, and

according to official reports, the astonishing number of 190,000 live plants were distributed in the short space of five years, to upwards of 2000 gardens in India, Europe, North and South America, North and South Africa, and Australia. In 1835 he undertook to conduct a deputation to inquire into the prospects of tea cultivation in Assam, a most unhealthy country, and on this occasion superintended a botanical exploration of the whole valley. On his return to Calcutta he was again obliged to leave India for his health, and he repaired to the Cape of Good Hope, much enfeebled and in a very critical state; there however, with a partial restoration to health, his latent zeal revived, and he accompanied our eminent Fellow, Mr. Maclear the Astronomer Royal, upon an extensive journey into the interior of the colony, botanizing diligently as he went, and transmitting his collections to Europe for distribution with his wonted liberality.

After another short sojourn at Calcutta and ineffectual attempt to resist the effects of a climate which five times drove him from India, Dr. Wallich finally returned to England in 1847, relinquished with regrets (that he never could banish) his arduous duties, and retired upon the pension of his rank as Surgeon in the Bengal army, after forty years of such incessant toil both of mind and body as has never been paralleled in the history of botanical science.

After his return to England Dr. Wallich's health gradually declined, but his love of botany and earnest desire to promote it never forsook him. He took an active part in the meetings of our own and other societies, and contributed, chiefly literary notices, to various botanical periodicals. He maintained an extensive correspondence and became a medium of communication between men of science in all its branches, in this country and on the Continent; and up to within a few weeks of his decease, which took place at his residence in Gower Street on the 28th of April, he was actively engaged in establishing a correspondence between the museums and gardens of his native and adopted countries. Dr. Wallich published several important works on systematic botany in India, and a magnificent one in this country, to which allusion has already been made; he has further the merit of having introduced the art of lithography into the East. His acquirements as a botanist were both varied and sound, and not confined to a familiarity with species;

for he possessed great knowledge of the habits, economic and medicinal properties and uses of plants, drew up many valuable reports on the agricultural products and forests of India, and was very extensively read in the literature of the science. He was further an elegant scholar, a classical writer, and an accomplished European and Oriental linguist. His loss has been deeply felt by botanists of all classes, for he was always contributing information and materials to those engaged in the study of the most abstruse as well as the most popular branches of the science. It may truly be said that no one ever applied to him in vain, nor has a book of any importance been published on botany in Europe within the last thirty years, in which Dr. Wallich's name is not prominently introduced.

Dr. Wallich was a Doctor of Medicine and of Philosophy, Fellow of the Royal Danish Society of Copenhagen, a Correspondent of the Academies of Sciences of Paris and Berlin, and a member of most of the learned bodies of Europe. He was a Vice-President of the Linnean Society of London, and a Knight-Commander of the Danish Order of Dannebrog. He was elected a Fellow of this Society in March 1829, and nominated a Vice-President in 1852.

On the motion of the Rev. Baden Powell, seconded by Dr. Warren, the best thanks of the Society were given to the President for his excellent Address, and his Lordship was requested to permit the same to be printed.

The Statutes relating to the election of Officers and Council having been read, and Dr. Roget and Thomas Webster, Esq. having, with the consent of the Society, been nominated Scrutators, the votes of the Fellows present were collected.

The following Noblemen and Gentlemen were reported duly elected Officers and Council for the ensuing year:—

President—The Lord Wrottesley, M.A.

Treasurer—Colonel Edward Sabine, R.A.

Secretaries— { William Sharpey, M.D.
George Gabriel Stokes, Esq., M.A.

Foreign Secretary—Rear-Admiral W. H. Smyth.

Other Members of the Council.—Neil Arnott, M.D.; Rear-Admiral

F. W. Beechey; Thomas Bell, Esq.; Sir Benjamin Brodie, Bart.; Charles Darwin, Esq., M.A.; Warren De la Rue, Esq.; The Earl of Harrowby; A. W. Hofmann, Ph. D.; Thomas Henry Huxley; Esq.; John Miers, Esq.; James Paget, Esq.; Rev. Baden Powell, M.A.; The Earl of Rosse, K.P., M.A.; Robert Stephenson, Esq.; William Tite, Esq.; Charles Wheatstone, Esq.

The following table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Honorary.	Foreign.	Having compounded.	Paying £2 12*s.* Annually.	Paying £4 Annually.	Total.
December 1, 1853..	11	47	416	14	273	761
Since elected......	......	+3	+6	......	+10	+19
Since compounded	......	......	+1	......	−1	
Defaulters	......	......	......	......	−2	− 2
Withdrawn	......	......	......	......	−1	− 1
Since deceased	−1	−3	−20	......	−8	−32
November 30, 1854	10	47	403	14	271	745

Statement of the Receipts and Payments of the Royal Society between December 1, 1853, and November 30, 1854.

Receipts	£	s.	d.
Balance from last year	1006	17	7
Subscriptions and Compositions	1664	8	0
Rents	251	4	6
Dividends on Stock	1054	4	3
Sale of Transactions and Proceedings	225	17	8
B. Oliveira, Esq., F.R.S.— Contribution to Donation Fund	50	0	0
	£4252	12	0

Payments	£	s.	d.
Rev. J. Ellis, Fairchild Lecture	3	0	0
Professor Graham, Bakerian Lecture	4	0	0
Salaries	707	10	0
Purchase of Stock	800	0	0
Fire Insurance	45	1	6
Printing Transactions, &c.	371	3	1
Engraving	54	14	5
Paper for Transactions	157	2	3
Binding Transactions	41	19	7
Books Purchased and Binding	275	9	11
Stationery	13	1	6
Shipping Expenses	11	3	0
Fire and Lighting (Two years)	122	15	0
House Expenses	101	10	4
Taxes	32	9	3
Completing Newton Collection	179	14	9
Rumford Fund	140	11	2
Wintringham Fund	34	19	0
Miscellaneous and Petty Charges	112	7	6
Balance	1043	19	9
	£4252	12	0

E. SABINE,
Treasurer.

March 1, 1855.

CHARLES WHEATSTONE, Esq., V.P., in the Chair.

The Earl of Ducie was admitted into the Society.

In accordance with the Statutes, the Secretary read the following list of Candidates for election into the Society.

Henry Foster Baxter, Esq.
George Bowdler Buckton, Esq.
Edward Chappell, Capt. R.N.
Arthur Connell, Esq.
William Coulson, Esq.
Thomas Russell Crampton, Esq.
Richard Cull, Esq.
Hugh Welch Diamond, M.D.
Solomon Moses Drach, Esq.
Major George Duckett.
William Farr, Esq.
William Lewis Fischer, Esq.
Isaac Fletcher, Esq.
Sir Charles Fox.
Rev. G. R. Gleig.
William John Hamilton, Esq.
Arthur Hill Hassall, M.D.
John Hawkshaw, Esq.
William Bird Herapath, M.D.
John Hippisley, Esq.
Henry Letheby, B.M.
Edward Joseph Lowe, Esq.
James Luke, Esq.
Robert Lutwidge, Esq.
George Macilwain, Esq.
William Marcet, M.D.
Robert William Mylne, Esq.
Henry Minchin Noad, Esq.
A. Follett Osler, Esq.
Henry Perigal, Esq.
Henry Hyde Salter, M.D.
Thomas Thomson, M.D.
Charles B. Vignoles, Esq.
Charles Vincent Walker, Esq.
Robert Wight, M.D.
Thomas Williams, M.D.
Alexander William Williamson, Esq.
George Fergusson Wilson, Esq.

The reading of Mr. Gosse's paper, "On the Structure, Functions, and Homology of the Manducatory Organs in the Class Rotifera," was resumed and concluded.

In this paper the author institutes an examination of the manducatory organs in the class Rotifera, in order to show that the various forms which they assume can all be reduced to a common type. He further proposes to inquire what are the real homologues of these organs in the other classes of animals, and what light we can gather, from their structure, on the question of the zoological rank of the Rotifera.

After an investigation of the bibliography of the class from Ehrenberg to the present time, in which the vagueness and inexactitude of our knowledge of these organs is shown, the author takes up, one by one, the various phases which they assume throughout the whole class; commencing with *Brachionus*, in which they appear in the highest state of development. Their form in this genus is therefore taken as the standard of comparison.

The hemispherical bulb, which is so conspicuous in *B. amphiceros*, lying across the breast, and containing organs which work vigorously against each other, has long been recognized as an organ of manducation: it has been called the gizzard; but the author proposes to distinguish it by the term *mastax*. It is a trilobate muscular sac, with walls varying much in thickness, receiving at the anterior extremity the *buccal funnel*, and on the dorsal side giving exit to the *œsophagus*.

Within this sac are placed two geniculate organs (the *mallei*), and a third on which they work (the *incus*). Each *malleus* consists of two parts (the *manubrium* and the *uncus*), united by a hinge-joint. The *manubrium* is a piece of irregular form, consisting of *carinæ* of solid matter, enclosing three areas, which are filled with a more membranous substance. The *uncus* consists of several slender pieces, more or less parallel, arranged like the teeth of a comb, or like the fingers of a hand.

The *incus* consists of two *rami*, which are articulated by a common base to the extremity of a thin rod (the *fulcrum*), in such a way that they can open and close by proper muscles. The fingers of

each *uncus* rest upon the corresponding *ramus*, to which they are attached by an elastic ligament. The *mallei* are moved to and fro by distinct muscles, which the author describes in detail; and by the action of these they approach and recede alternately; the rami opening and shutting simultaneously, with a movement derived partly from the action of the *mallei*, and partly from their own proper muscles.

All these organs have great solidity and density; and, from the action of certain menstrua upon them, appear to be of calcareous origin.

The writer proceeds to describe the accessory organs. The ciliated disc has an infundibuliform centre, which commonly merges into a tube before it enters the *mastax*. The particles of food that float in the water, or swimming animalcules, are whirled by the ciliary vortex into this tube; and, being carried into the *mastax*, are lodged upon the *rami*, between the two *unci*. These conjointly work upon the food, which passes on towards the tips of the *rami*, and enter the *œsophagus*, which opens immediately beneath them.

From this normal condition, the author traces the manducatory organs through various modifications, in the genera *Euchlanis*, *Notommata aurita*, *N. clavulata*, *Anuræa*, *N. petromyzon*, *N. lacinulata*, *Furcularia*, *N. gibba*, *Synchæta*, *Polyarthra*, *Diglena*, *Eosphora*, *Albertia*, *F. marina*, *Asplanchna*, *Mastigocerca*, *Monocerca*, and *Scaridium*. Some of these display peculiarities and aberrations highly curious. Notwithstanding the anomalies and variations which occur, however, the same type of structure is seen in all; and the modifications in general may be considered as successive degenerations of the *mallei*, and augmentations of the *incus*.

The form of the manducatory organs, which occurs in *Triarthra*, *Pompholyx*, *Pterodina*, *Œcistes*, *Limnias*, *Melicerta*, *Conochilus*, *Megalotrocha*, *Lacinularia*, and *Tubicolaria*, is next examined. The organs are shown to be essentially the same as in the former type, but somewhat disguised by the excessive dilatation of the *mallei*, and by the soldering of the *unci* and the *rami* together, into two masses, each of which approaches in figure to the quadrant of a sphere.

Attention is then directed to what has been called (but by a misapprehension) the "stirrup-shaped" armature of the genera *Rotifer*,

Philodina, *Actinurus*, &c. Here, however, the organs are proved to have no essential diversity from the common type; their analogy with those last described being abundantly manifest, though they are still further disguised by the obsolescence of the *manubria*.

Floscularia and *Stephanoceros*, the most elegant, but the most aberrant forms of Rotifera, close the series. The *mastax*, in these genera, is wanting; and in the former genus the *incus* and the *manubria* are reduced to extreme evanescence, though the two-fingered *unci* show, in their structure, relative position and action, the true analogy of these organs.

Having thus shown that there is but one model of structure, however modified or disguised, in the manducatory organs of the Rotifera, the author proceeds to the question of their homology. He argues on several grounds that they have no true affinity with the gastric teeth of the Crustacea, though he states his conviction that the Rotifera belong to the great Arthropodous division of animals.

It is with the Insecta that the author seeks to ally these minute creatures; and, by a course of argument founded on the peculiarities of structure already detailed, he maintains the following identifications:—that the *mastax* is a true *mouth*; that the *mallei* are *mandibles*; the *manubria* possibly representing the *cheeks*, into which they are articulated; that the *rami* of the *incus* are *maxillæ*; and that the *fulcrum* represents the *cardines* soldered together.

While the author maintains the connexion of Rotifera with Insecta, through these organs in their highest development, he suggests their affinity with Polyzoa, by the same organs at the opposite extremity of the scale, since the oval muscular bulbs in *Bowerbankia*, which approach and recede in their action on food, seem to represent the quadriglobular masses of *Limnias* and *Rotifer*, further degenerated.

If this affinity be correctly indicated, the interesting fact is apparent, that the Polyzoa present the point where the two great parallel divisions, Mollusca and Articulata, unite in their course towards the true Polypi.

Mr. Gosse's paper is illustrated by ninety-six figures of entire Rotifera, or of the parts under review, all drawn from the life, and, for the most part, with a power of 560 diameters.

March 8, 1855.

Sir BENJAMIN BRODIE, Bart., V.P., in the Chair.

The following papers were read:—

I. "On the Perihelia and Nodes of the Planets." By EDWARD J. COOPER, F.R.S. Received February 2, 1855.

Prefatory to my volume on Cometic Orbits, published in 1852, I invited the attention of astronomers to the several points of resemblance between the planetary orbits and those of periodic comets. Among these it was shown, that of the heliocentric longitudes of perihelia and ascending nodes of the then known planets and periodic comets, two-thirds were situated in the heliocentric semicircle between 315° and 135°. The planets stood thus in quadrants—

$$\text{L. P.'s between}\ \left.\begin{array}{l} 45^\circ \text{ and } 135^\circ = \qquad\qquad 9 \\ \left.\begin{array}{l}135 \text{ and } 225=4\\ 225 \text{ and } 315=3\end{array}\right\}7 \\ 315 \text{ and } \ 45= \qquad\qquad 7\end{array}\right\}16.$$

$$☊ \quad \text{between}\ \left.\begin{array}{l} 45 \text{ and } 135 = \qquad\qquad 13 \\ \left.\begin{array}{l}135 \text{ and } 225=4\\ 225 \text{ and } 315=4\end{array}\right\}8 \\ 315 \text{ and } \ 45= \qquad\qquad 1\end{array}\right\}14.$$

Here the L. P.'s appeared as 16 to 7, and the ascending nodes as 14 to 8. Two additional asteroids were subsequently discovered leaving the L. P.'s as 16 to 9, and the ascending nodes as 15 to 9.

Again, in 1853, I sent a note upon the same subject to the Royal Astronomical Society of London. At that time a considerable addition had been made to the asteroids, and the total number of planets had risen from 25 to 35. Following the same distribution of the

perihelia and ascending nodes as in my previously published work, the result was—

$$\text{L. P.'s between}\quad \left.\begin{array}{l} 45^\circ \text{ and } 135^\circ = \qquad 13 \\ \left.\begin{array}{l}135 \text{ and } 225 = 5\\ 225 \text{ and } 315 = 6\end{array}\right\}11 \\ 315 \text{ and } \ \ 45 = \qquad 11\end{array}\right\}24.$$

$$☊\quad\text{between}\quad \left.\begin{array}{l} 45 \text{ and } 135 = \qquad 19 \\ \left.\begin{array}{l}135 \text{ and } 225 = 9\\ 225 \text{ and } 315 = 4\end{array}\right\}13 \\ 315 \text{ and } \ \ 45 = \qquad 2\end{array}\right\}21.$$

The suspicion of some yet undiscovered law became strengthened by this further investigation; and it occurred to me to ascertain if any other heliocentric semicircles would mark the effect of such law more clearly. Let me be permitted to extract the concluding passage from the note as it is printed in the Royal Astronomical Society's Notices:—

"But if, instead of the semicircles 315° to 135° and 135° to 315°, we adopt those from 45° to 225° and 225° to 45°, we see that of the ascending nodes of thirty-four planets, twenty-eight are found in the first semicircle and only six in the second. Again, the semicircles that contain the greatest number of L. P.'s of planets are between 0° and 180°, or 10° and 190°. That which contains the greatest number of nodes is between 35° and 215°. In the first case there are twenty-six, and in the latter twenty-nine. The quadrant containing the largest number of L. P.'s of planets is that between 11° and 101°, of which there are sixteen. That containing the largest of nodes is from $35\frac{1}{2}$° to $125\frac{1}{2}$°, of which there are twenty."

At the present moment (January 1855) we have orbits, more or less accurate, of forty-one planets. It cannot be altogether uninteresting to pursue once more the traces of a law still unknown, if it have existence. Our position now stands thus—

$$\text{L. P.'s between}\quad \left.\begin{array}{l} 45^\circ \text{ and } 135 = \qquad 16 \\ \left.\begin{array}{l}135 \text{ and } 225 = \ 6\\ 225 \text{ and } 315 = \ 6\end{array}\right\}12 \\ 315 \text{ and } \ \ 45 = \qquad 13\end{array}\right\}29.$$

$$\text{☊ between } \left.\begin{array}{l} 45 \text{ and } 135 = \qquad\qquad 19 \\ \left.\begin{array}{l}135 \text{ and } 225 = 11\\ 225 \text{ and } 315 = 5\end{array}\right\} 16 \\ 315 \text{ and } 45 \qquad\qquad\quad 5\end{array}\right\} 24.$$

But, be it remembered, that in 1853 of the then known planets the greatest number of L. P.'s were found to be situated in the heliocentric semicircles 0° to 180° or 10° to 190°. At present we shall find the perihelia of thirty out of the forty-one planets in either of these semicircles. The greatest number of nodes were then (1853) between 35° and 315° = 29; and 45° and 225° = 28. At present, of forty planets there are thirty nodes in either of these heliocentric semicircles. These facts are at least very singular. I may tabulate them—

Of forty-one planets, L. P.'s between	0° and 180° = 30
	10 and 190 = 30
Of forty planets ☊ between	35 and 215 = 30
	45 and 225 = 30
and between 354° to 355° and	174 to 175 = 31

We here perceive that there are thirty L. P.'s situated in the heliocentric semicircle between 0° to 10° and 180° to 190°. It is also the fact, that there are thirty ascending nodes between 357° to 7° and 177° to 187°, which may be called the same semicircle as that in which the thirty L. P.'s are found.

The quadrant containing the greatest number of L. P.'s of the forty-one planets, is that between 10° and 100° = 20.

Those containing the greatest number of ascending nodes, are

between 36° to 43° and 126° to 133° = 20
and between 62 to 66 and 152 to 156 = 20.

Surely there must be an undiscovered cause determining the orbits in this way. Having laid these facts before my first assistant Mr. Graham, he computed the degree of probability of such a law, arguing thus:—"Were the nodes and perihelia indifferent to all heliocentric longitudes, it would of course be an equal chance in the case of a planet whose orbit had not been determined, in which semicircle either would be found; and the *à priori* probability that,

of the forty-one known L. P.'s, thirty would be in one semicircle, is about $\frac{1}{696}$; and that of the forty ascending nodes, thirty-one would be in one semicircle, is about $\frac{1}{4021}$. Thus the probability that there is some influence causing a tendency to one semicircle, ascertained from the facts before us, is very strong: for, for the L. P.'s, the odds are about 660 to 1, and for the ascending nodes about 4430 to 1 in favour of such a supposition." But after all it may be an accidental coincidence; as, consistently with the laws of planetary motion, such a congregation of perihelia or nodes may occur at periods exceedingly remote. The further consideration of this subject must be left to analysts, of leisure and inclination to pursue it.

II. "On Circumstances modifying the Action of Chemical Affinity." By J. H. Gladstone, Ph.D., F.R.S. Received February 1, 1855.

The question intended to be solved in this communication is,—what takes place when two binary compounds AB and CD are brought together under such circumstances that both they themselves and the products of their mutual action remain free to react? Do they, according to a generally received opinion, remain unaltered, or, should the affinities so preponderate, become simply AB and CB? Or do A and C, according to Berthollet's view, divide themselves in certain proportions between B and D, the said proportions being determined not solely by the difference of energy in the affinities, but also by the difference of the quantities of the bodies? And, supposing the latter to be the correct view, do the amounts of AD and CB produced by the reaction, increase progressively with the relative increase of AB, or do sudden transitions occur, such as Bunsen and Debus have recently observed in certain cases where the products were removed at once from the field of action?

A reply was sought in the colours produced upon mixing different salts in aqueous solution. There were not many coloured salts suitable for the purpose, as it generally happens that a base gives the same colour with whatever acid it is combined, and *vice versâ*; but the compounds of sesquioxide of iron were peculiarly adapted to

the requirements of the experiment, as some are intensely coloured while others are nearly colourless.

The circumstances that attended the formation of the blood-red sulphocyanide were first fully examined. On mixing known quantities of different ferric salts with known quantities of different sulphocyanides, it was found that the whole of the iron was never converted into the red salt; that the amount of it so converted depended on the nature both of the acid combined with the ferric oxide, and of the base combined with the sulphocyanogen; and that it mattered not how the bases and acids had been combined previous to their mixture, so long as the same quantities were brought together into solution. The effect of mass was fully tried by mixing equivalent proportions of ferric salts and sulphocyanides, and then adding known amounts of either one or the other compound. It was found that in either case the amount of red salt was increased; and that when the numbers of equivalents of the salt added were taken as abscissæ, and the amounts of red sulphocyanide produced, as ordinates, the numbers observed in the experiments gave regular curves, though not belonging to the second order. The curves representing the experiments in which sulphocyanide of potassium was mixed with ferric nitrate, chloride, or sulphate, appeared to be the same, but hydrosulphocyanic acid gave a different curve. The deepest colour was given when nitrate of iron was mixed with the sulphocyanide, but even upon the admixture of one equivalent of the former with three of the latter, only 0·194 equiv. of the intensely red ferric salt was formed, and when 375 equivalents of sulphocyanide of potassium had been added there was still a recognizable amount of nitrate of iron undecomposed. It was found that the addition of a colourless salt not only reduced the colour of a solution of ferric sulphocyanide, but also that the reduction increased in a regularly progressive ratio according to the mass of the salt.

Other ferric salts were likewise examined. The black gallate gave results precisely analogous to those obtained by means of the sulphocyanide; the red meconate also confirmed Berthollet's views, but the action of mass was rendered obscure by the formation of double or of acid salts; the red pyromeconate resembled the meconate; the red acetate bore similar testimony; the blue solution of the ferric ferrocyanide in oxalic acid gave results fully corroborative

of the influence both of the nature and of the mass of every substance present at the same time in the mixture; the purple and the red comenamate afforded similar results; while the red bromide (not the oxybromide), though somewhat indistinct in its testimony, corroborated to a certain extent the preceding observations.

Experiments were performed with a view to determine what effect the mass of water might have on the salts operated upon; its influence in reducing the colour of the ferric sulphocyanide was found to be very great, but the nature of it could not be exactly determined. As however it was uniform in its action in whatever manner the sulphocyanide had been produced, it could not affect the results of the preceding experiments. Water did not appear to act in any similar manner upon the other ferric salts.

From the mass of quantitative observations made during the investigation, it was possible to deduce not only the order of affinity of the various acids for sesquioxide of iron as compared with potash, but also to assign approximative numbers. Doubt may rest on the position of some terms in the series, but hydrosulphocyanic acid certainly had the least affinity for ferric oxide in comparison with potash: it was represented by unity: the other acids followed in the order—nitric, 4; hydrochloric, 5; sulphuric, 7; gallic, 10; pyromeconic; meconic; acetic, 20; hydrobromic; comenamic; citric, 100; hydroferrocyanic, 170.

Other coloured salts were submitted to a more cursory investigation. The scarlet bromide of gold when treated with an alkaline chloride gave a striking instance of the effect of mass in gradually overcoming a strong affinity. The intensely red iodide of platinum afforded results which, though somewhat obscure, were not opposed in their testimony. So did the blue sulphate of copper when treated with different chlorides. The "manganoso-manganic oxide" dissolves in sulphuric or phosphoric acid of a red, and in other acids of a deep brown colour; and it was found that hydrochloric acid was capable of changing the colour of the sulphate according to its mass, while on the other hand sulphuric or phosphoric acid altered in like manner the tint of the chloride. Somewhat similar results were obtained by means of the green chloride and the purple fluoride of molybdenum; and the blue solution that forms when gallic acid is brought in contact with both the oxides of iron at once, bore testi-

mony to the same general laws. The peculiar optical character of certain salts of quinine was also taken advantage of for determining what changes took place among the compounds in solution. The amount of fluorescence exhibited by a solution of acid sulphate of quinine was found to be affected by the admixture of a chloride, bromide, or iodide according to the nature and the mass of the salt added, and the addition of sulphuric, phosphoric, nitric and other acids was found to produce a fluorescence in solutions either of hydrochlorate of quinine, or of sulphate which had been rendered non-fluorescent by hydrochloric acid. Similar results were obtained with quinidine; and somewhat analogous ones with the organic bases contained in horse-chestnut bark, and in tincture of stramonium. An experiment is also narrated showing that the same laws hold good in respect to compound ethers as to salts having metallic bases, alcohol being employed as the solvent.

Beside the very diversified substances already mentioned in this abstract, several others, such as lead, mercury, zinc, potash, soda, baryta, lime, and ammonia, are shown by a more indirect proof to enter into compounds which obey the same laws. Hence it is concluded that what was observed in reference to the ferric salts holds good very generally, if not universally.

The bearing of certain other phenomena upon the question at issue was also examined. The fact that precipitation, when it occurs, gives rise to a perfect interchange of bases and acids, is equally consistent with either Bergmann's or Berthollet's theory; but not so is the fact that two soluble salts cannot be mixed without the occurrence of precipitation, if one of the products that may be formed is an insoluble salt. The only recorded exception to this law, which occurs with oxalate of iron in the presence of a salt of yttria, under peculiar circumstances, was found on close examination to be in perfect accordance with the principles laid down by Berthollet. Besides the argument founded on this universal fact, several experiments were devised for the purpose of proving that the complete precipitation of an insoluble salt on the mixing of two soluble salts, was due to the insoluble compound being removed at once out of the field of action on the first distribution of the elements, thus necessitating a redivision, and so on until no more of it could possibly be formed. The phenomena attending volatilization have the same

bearing as those connected with precipitation. If by the mutual action of two salts a substance be formed, which, though soluble in water, requires more water for its solution than is present, it crystallizes out: certain experiments were noted where this action occurs, and it was found that they gave testimony in favour of the same views as have been supported by the preceding observations. The bearing of the phenomenon of diffusion of salts upon the point at issue was also examined: Malaguti's experiments were discussed; and they, as well as some observations on the solution of certain bodies by others set at liberty, were found to bear testimony also in the same direction.

During the whole of the experiments on this subject, most of which were performed quantitatively, no unequivocal instance occurred of two substances having so strong an affinity for one another, that they combined to the exclusion of other bodies of like kind present in the same solution. After showing that some reputed exceptions are really not capable of being proved to be so, and after suggesting some probable limitations of the action of the general law, the paper concludes with the following deductions:—

I. That where two or more binary compounds are mixed under such circumstances that all the resulting compounds are free to act and react, each electro-positive element enters into combination with each electro-negative element in certain constant proportions.

II. That these proportions are independent of the manner in which the different elements were primarily arranged.

III. That these proportions are not merely the resultant of the various strengths of affinity of the several substances for each other, but are dependent also on the mass of each of the substances present in the mixture.

IV. That an alteration in the mass of any one of the binary compounds present alters the amount of every one of the other binary compounds, and that in a regular progressive ratio; sudden transitions only occurring where a substance is present which is capable of combining with another in more than one proportion.

V. That this equilibrium of affinities arranges itself in most cases in an inappreciably short space of time, but that in certain instances the elements do not attain their final state of combination for hours.

VI. That totally different phenomena present themselves where

precipitation, volatilization, crystallization, and perhaps other actions occur, simply because one of the substances is thus removed from the field of action, and the equilibrium that was first established is thus destroyed.

VII. That consequently there is a fundamental error in all attempts to determine the relative strength of affinity by precipitation,—in all methods of quantitative analysis founded on the colour of a solution in which colourless salts are also present,—and in all conclusions as to what substances exist in a solution, drawn from such empirical rules as, that "the strongest acid combines with the strongest base."

March 15, 1855.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read:—

I. "Researches on Organo-metallic Bodies." By E. FRANKLAND, Ph.D., F.R.S., Professor of Chemistry in Owens College, Manchester. Second Memoir.—Zincethyl. Received February 9, 1855.

This compound, whose existence was mentioned in a previous memoir*, is formed by the action of zinc upon iodide of ethyl, or a mixture of iodide of ethyl and anhydrous ether, at a temperature exceeding 100° C. The materials are enclosed in a copper digester capable of resisting great pressure. When purified by rectification in an atmosphere of carbonic acid, zincethyl possesses the following properties:—At ordinary temperatures it is a colourless, transparent and mobile liquid, refracting light strongly and possessing a peculiar odour, rather pleasant than otherwise, and therefore differing greatly from that of zincmethyl. Its specific gravity is 1·182 at 18° C. Exposed to a cold of $-22°$ C. it exhibits no tendency to become

* Philosophical Transactions, 1852, p. 436.

solid. Zincethyl boils at 118° C., and distils unchanged. The specific gravity of its vapour is 4·251. Several analyses of zincethyl prove its formula to be

$$C_4 H_5 Zn.$$

The vapour volume of zincethyl is highly remarkable, and almost compels us to conclude that the vapour volume of the double atom of zinc is only equal to that of oxygen, instead of corresponding with the volume of hydrogen, in accordance with the generally received supposition. Zincethyl, therefore, appears to belong to the so-called water type, and to consist of two volumes of ethyl and one volume of zinc vapour; the three volumes being condensed to two: for if we were to assume that an equivalent of zinc occupies the same vapour volume as an equivalent of hydrogen, we should then have the anomaly of the combination of equal volumes of two radicals being attended by condensation.

Although zincethyl is remarkable for the intense energy of its affinities, which place it nearly at the head of the list of electro-positive bodies, yet it does not appear to be capable of forming any true compounds with electro-negative elements, its reactions being all double decompositions in which the constituents of the zincethyl separate. Zincethyl is spontaneously inflammable in atmospheric air or oxygen; but when a few drops, diluted with ether to prevent inflammation, are passed into a mercurial eudiometer containing dry atmospheric air, a rapid absorption of oxygen takes place, with the formation of a white amorphous solid composed of zinc, ethyl, and oxygen. This reaction, which is also common to zincmethyl and zincamyl, led me to suppose that, like cacodyl, these bodies combined directly with oxygen; but the results of a closer study of the action of oxygen upon zincethyl prove that no such compound is formed; the white body being ethylate of zinc, and containing no organo-metallic compound, in the strict sense of the term. The action of oxygen upon zincethyl is expressed in the following equation:—

$$\left.\begin{array}{c} C_4 H_5 Zn \\ O, O \end{array}\right\} = C_4 H_5 O\, ZnO.$$

The ethylate of zinc thus produced is decomposed by water into hydrated oxide of zinc and alcohol—

$$\left.\begin{array}{r} Zn O\, C_4 H_5 O \\ 2H O \end{array}\right\} = \left\{\begin{array}{r} C_4 H_5 O\, HO \\ Zn O\, HO. \end{array}\right.$$

Zincethyl is acted upon with great energy by iodine; when the violence of the reaction is moderated, by the application of intense cold and the intervention of ether, the sole products are iodide of zinc and iodide of ethyl—

$$\left.\begin{matrix} C_4\,H_5\,Zn \\ I, I \end{matrix}\right\} = \left\{\begin{matrix} C_4\,H_5\,I \\ Zn\,I. \end{matrix}\right.$$

Bromine acts with explosive violence on zincethyl, but the action may be moderated by adding the bromine in the form of diffused vapour and cooling to 0° C. The sole products of the reaction are then bromide of ethyl and bromide of zinc—

$$\left.\begin{matrix} C_4\,H_5\,Zn \\ Br, Br \end{matrix}\right\} = \left\{\begin{matrix} C_4\,H_5\,Br \\ Zn\,Br. \end{matrix}\right.$$

Zincethyl burns with a lurid flame spontaneously in chlorine gas; the zinc and hydrogen are converted into chlorides, whilst carbon is deposited in the form of soot. I have not studied the products of a more moderate action, as it is difficult to bring the materials together without too great an elevation of temperature. There can be no doubt, however, that the moderated action of chlorine would be analogous to that of bromine or iodine, and that the products would be chloride of ethyl and chloride of zinc—

$$\left.\begin{matrix} C_4\,H_5\,Zn \\ Cl, Cl \end{matrix}\right\} = \left\{\begin{matrix} C_4\,H_5\,Cl \\ Zn\,Cl. \end{matrix}\right.$$

Carefully dried flowers of sulphur have only a slight action upon an ethereal solution of zincethyl, but the application of a gentle heat suffices to produce a brisk reaction; the sulphur gradually disappears, a white flocculent precipitate is formed, and a strong odour of sulphide of ethyl developed. The chief product of this reaction is the double sulphide of ethyl and zinc (mercaptide of zinc), which is produced as follows:—

$$\left.\begin{matrix} C_4\,H_5\,Zn \\ S, S \end{matrix}\right\} = C_4\,H_5\,S + Zn\,S.$$

A little free sulphide of ethyl is also formed—

$$\left.\begin{matrix} C_4\,H_5\,Zn \\ S, S \end{matrix}\right\} = \left\{\begin{matrix} C_4\,H_5\,S \\ Zn\,S. \end{matrix}\right.$$

Finally, zincethyl is decomposed by water into oxide of zinc and hydride of ethyl—

$$\left.\begin{matrix} C_4H_5Zn \\ HO \end{matrix}\right\} = \left\{\begin{matrix} C_4H_5H \\ ZnO. \end{matrix}\right.$$

It is also similarly acted upon by the hydrated acids and by the hydrogen compounds of chlorine, bromine, iodine, fluorine and sulphur.

The behaviour of zincethyl in contact with the electro-negative elements is highly remarkable, and cannot fail to have an important influence upon our views of the condition of bodies at the moment of chemical change,—a subject so ably discussed by Brodie*, whose ingenious views I consider to receive a new support in these reactions of zincethyl, by the singular way in which ethyl, a body low down in the electro-positive series, unites with oxygen, chlorine, &c., in the presence of a large excess of the intensely electro-positive zincethyl. This behaviour also strikingly confirms the suggestions I ventured to make in my former memoir†, relative to the moleculo-symmetrical form of organo-metallic compounds. In the inorganic combinations of zinc, this metal unites with one atom only of other elements; a very unstable peroxide, not hitherto isolated, being the only exception. The atom of zinc appears, therefore, to have only one point of attraction, and hence, notwithstanding the intense affinities of its compound with ethyl, any union with a second body is necessarily attended by the expulsion of the ethyl.

II. "Note on the Magnetic Medium." By Prof. A. W. Williamson. Communicated by Dr. Sharpey, Sec. R.S. Received March 15, 1855.

In a letter to Mr. Faraday recently published in the Philosophical Magazine, Dr. Tyndall brings forward some important considerations on the subject of magnetic philosophy.

It has been known for some time that the phenomena of diamagnetism may be produced artificially in bodies which are usually

* Philosophical Transactions, 1850, p. 789.

† Ibid. 1852, p. 438.

considered magnetic. For this purpose it is only necessary to plunge the magnetic body into a yielding medium more magnetic than itself. When thus exposed to the action of a magnet it recedes from the poles, because the volume of the medium which it displaces is more powerfully attracted.

This fact naturally suggested the idea, that all repulsion by the magnet might be owing to the attraction exercised on the medium being stronger than that on the body repelled;—just as balloons are driven upwards by the superior weight of the displaced volume of air. And as phenomena of diamagnetism are observed in a so-called vacuum, it was thought that some "magnetic medium" might be present there.

I do not purpose on this occasion to enter upon the general question of the evidence which may be adduced for or against this important conclusion; for it could only be proved satisfactorily by considerations including phenomena of the most varied kind, such as electricity, light, chemical action, &c., to which it must necessarily apply. But it might be disproved by any one well-understood fact contradictory to it.

Now it appears to me, that the facts adduced by Dr. Tyndall are not inconsistent with the notion of a magnetic medium, but follow naturally from it; and that his argument involves a tacit assumption foreign to the theory under consideration.

The first fact adduced is, that compression increases the attraction of magnetic bodies, and the repulsion of diamagnetic bodies by the magnet, in the direction of the line of compression. Now it is evident, that a variation of pressure on a number of particles surrounded by a magnetic medium may alter the attraction of the mass by a magnet in two ways;—first, by altering the density of the matter*; secondly, by altering the density of the medium.

In a cubical mass of carbonate of iron the material particles are more magnetic than the medium which they displace, and the force with which it is attracted is proportional to this excess.

If it becomes more magnetic by compression, we must conclude that the loss of magnetic medium *from its interstices* is more than supplied by the magnetic matter which takes its place.

Carbonate of lime is less magnetic than the quantity of medium

* The word "matter" is here used for brevity to denote *ponderable* matter.

which its particles displace, and when these particles are brought closer together by pressure, with diminution of the intervening spaces occupied by the medium, the mass becomes more diamagnetic, because a certain quantity of the magnetic medium is thus replaced by the less magnetic matter.

Dr. Tyndall seems to have assumed, that on the compression of an aggregate of particles of a diamagnetic substance, the medium is not displaced by the particles in their change of position;—in which case his conclusion, that compression must increase the magnetic functions of *every* substance, would no doubt follow from the notion of a magnetic medium.

The second fact adduced differs chiefly in form from the one just considered. Crystals of carbonate of iron are attracted most strongly by a magnet acting in the direction of the crystallographic axis. Crystals of carbonate of lime, possessing the same form, are most strongly repelled in the direction of the same axis. In this direction the functions of the matter predominate more over those of the medium than in other directions of the crystal; so that with carbonate of iron, we have the strongest magnetism; with carbonate of lime, the strongest diamagnetism in this axis. One crystal consists of magnetic medium with strongly magnetic matter; the other consists of the medium with matter of very slight magnetic force.

The crystallographic axis is in both crystals the direction in which the function of matter predominates most strongly over that of the medium: so that in the iron salt it is the most magnetic; in the lime salt the feeblest magnetic direction in the crystal.

March 22, 1855.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read:—

I. "Further observations on the Anatomy of *Macgillivraya*, *Cheletropis*, and allied genera of pelagic Gasteropoda." By JOHN DENIS MACDONALD, Esq., R.N., Assistant-Surgeon H.M.S.V. 'Torch.' Communicated by Sir W. BURNETT, K.C.B. Received February 22, 1855.

The author states, that in a late voyage from Sydney to Moreton Bay, specimens of *Macgillivraya, Cheletropis* and a few other genera of minute pelagic Gasteropoda, apparently undescribed, were daily taken in the towing-net, and afforded him an opportunity of more precisely determining the mode of attachment of the ciliated arms which he had at first presumed to be naked branchiæ.

In his former paper* it was stated, more particularly of *Cheletropis Huxleyi*, that the gills were of two kinds, viz. "covered" and "naked;" the former, corresponding to those of the pectinibranchiate Gasteropoda generally, he has never found to be absent in any of the genera; but from further observation of the so-called naked gills, while the animals were alive in their native element, he is disposed to think that they are chiefly employed for prehension, and probably as auxiliary organs of natation. When these ciliated appendages are fully extended, the line of cilia is perfectly straight, so that the frilled border, noticed in the previous account, turns out to be a character depending simply on the partial contraction of the longitudinal muscular fibres, preparatory to complete retraction of the organs. They have no connexion with the mantle, but encircle

* Proceedings, p. 191.

the mouth together with the tentacula and eyes, and coalesce at their bases like the segments of a deeply-cleft calyx. In the specimens of *Macgillivraya* examined the arms were quite transparent, but marked at irregular intervals with cross streaks of brownish purple. In the extended state they were several times the length of the shell, and, like the arms of a polype, they rolled themselves up when touched, and started back into the shell with surprising rapidity. They appeared also to be exquisitely sensitive, exhibiting short twitching movements when minute particles suspended in the water came in contact with them.

In the specimens of *Macgillivraya* now referred to, the respiratory siphon consisted of a process of the mantle converted into a tube by the mere apposition of its borders without organic union; it was moreover much shorter than had been usually observed in previous examples, and the author thinks that those now under consideration may be a variety, if not a distinct species.

In his former examinations of this tribe of Gasteropoda, the author had never found more than four arms encircling the head, but he has since discovered six in a single genus with which he had been long familiar by external characters. In this case the operculigerous lobe of the foot is quite cylindrical and of some length, bearing the peculiar operculum on its truncated extremity with the clawed process pointing to the left side. The sucker-disc is very small, and presents an anterior and posterior lobe. The two tentacula bear each an ocellus on the outer side near the base, and the ciliated arms, in every respect save number, resemble those of *Macgillivraya* and its congeners. The clawed operculum is developed from a spiral nucleus situate near the internal thickened border; it seems to be a weapon of defence, and is wielded with great dexterity by the little animal, which makes skips and jerks by means of its complex foot, after the manner of *Nassa* or *Strombus*.

The author notices another member of this diminutive tribe which is very commonly met with in the South Pacific, and has almost an indefinite range. As regards both animal and shell, it in many points resembles a miniature *Natica*. The shell is few-whorled, with small compressed spire and ventricose mouth; the operculum paucispiral and well-marked with the lines of growth. The foot is not unlike a broad and square-toed shoe in form, receiving or bearing

the remainder of the animal and the shell. The shoe-upper, as it were, presents two rounded lateral lobes which lie over the anterior part of the shell, like the mentum of *Natica*. The little animal creeps on its foot with great rapidity, appearing rather to slide along than progress by a vermicular movement, and by spreading out and hollowing this organ at the surface of the water, as a freshwater Lymnæad forms a boat of its foot, it buoys up its tiny body and is cast abroad on the face of the ocean.

The paper was illustrated with coloured figures of most of the objects described.

II. On the Anatomy of *Nautilus umbilicatus*, compared with that of *Nautilus Pompilius*." By JOHN DENIS MACDONALD, Esq., R.N. Communicated by Sir W. BURNETT, K.C.B. Received February 22, 1855.

During a visit of H.M.S.V. 'Torch' to the Isle of Pines in July 1854, a recent specimen of *Nautilus umbilicatus* was picked up on the outer reef off Observatory Island. It was alive when brought on board, but was too much exhausted to exhibit active movements. Part of the hood appeared to have been eaten away behind by some predaceous enemy, but in other respects the animal was perfect.

The body when retracted lay more deeply in the shell than that of *N. Pompilius*, so that no part was visible in a lateral view, and on account of the great depth of the chamber of occupation the orifice of the siphuncle in the last septum could not be seen when the soft parts were removed. As to this difference, however, the author observes that it may depend on the time elapsed since the formation of the last partition.

Apart from the shells, the author finds a close resemblance between the corresponding parts of the two species.

The specimen of *N. umbilicatus* examined proved to be a female; a fact which may serve to modify the views of those who, adopting the speculations of D'Orbigny on the sexes of the Ammonites as indicated by the characters of their shells, apply them also to the several kinds of *Nautili* known.

The body of *N. umbilicatus* is larger and more elongated than that of *N. Pompilius* as it occurs in the South Seas, although the specimens of the latter species brought from the Chinese Seas much exceed both in size. In the *N. umbilicatus*, the longitudinal lamellæ on the median lobe of the external labial processes are divided by a wide groove into two lateral sets, and the corresponding lamellæ between the internal labial processes are about seventeen in number and of considerable thickness. In *N. Pompilius*, the latter lamellæ are much thinner and more numerous, and the lateral sets of the former are united together in the median line, commencing anteriorly with an azygos transverse lamina. In both kinds, however, the corresponding tentacula may be distinctly traced out, with only such minor differences as might be expected to occur in different specimens of either separately; the digital, labial and ocular groups agreeing sufficiently both as to number and character in the two cases, considering the liability of these parts to slight modifications, from arrest of development or redundance, in the same species.

Referring to former observations of his own on the eye of *N. Pompilius*, the author observes that they closely apply to *N. umbilicatus*, which affords confirmation of his opinion that the pigmentary coating is subjacent to the retina. He finds no vestige of a lens, and in place of vitreous humour, a mere viscous matter protecting the retina from the sea-water.

The organ of hearing, which had escaped detection in the specimen of *N. Pompilius* dissected by Professor Owen, altered as it doubtless had been by long immersion in spirit, was discovered in the example of *N. umbilicatus* examined by the author. It consists of two spheroidal acoustic capsules placed, one on each side, at the union of the supra and subœsophageal ganglia, and measuring about one-twelfth of an inch in diameter. Each capsule rests internally against the nervous mass, and is received on its outer side into a little depression in the cephalic cartilage. It is enveloped in a kind of fibrous tissue and filled with a cretaceous pulp consisting of minute, elliptical, otoconial particles, presenting under a high power a bright point near each end, varying much in size, and sometimes combined into stellate, cruciform or other figures. Cilia were not observed within the capsules.

The inside of the mouth is furnished with three groups of papillæ,

one of which occupies the median line between the orifice of the tongue-sac and commencement of the œsophagus. These lingual papillæ, as well as the rest, are clothed with long and slender columnar epithelium-particles.

The author agrees with Mayer in regarding the well-known follicular appendages of the afferent branchial vessels of the Cephalopoda, as performing the function of kidneys, but admits that they may also serve, by altering their capacity, to regulate the amount of blood passing through the branchiæ under changes of pressure to which the animal may be subjected at different depths. These follicles are subcylindrical in form, and somewhat dilated at the free extremity, to which is appended a folded and funnel-shaped process of membrane which expands rather suddenly and presents a jagged border. They open by an oval or slit-like orifice into the afferent branchial vessels, on each of which, as Professor Owen has observed, they are disposed in three clusters. The outer membrane is smooth and glossy, homogeneous in structure, and sprinkled over with minute, rounded, transparent bodies, resembling the nuclei of cells. Beneath this layer, flat bundles of fibres, apparently muscular, are traceable here and there, principally disposed in a longitudinal direction, and sometimes branched. The lining membrane consists of a loose epithelial pavement, similar in many respects to that of the uriniferous tubules of the higher animals, the cells containing, besides the nuclei, numerous minute oil-globules, or a substance much resembling concrete fatty matter. This membrane is thrown up into very numerous papillæ and corrugations, so as greatly to increase the extent of surface. The papillæ are more numerous towards the attached end, and a circlet of longitudinal folds, with transverse zigzag corrugations, radiate from the bottom of the follicle, in which a number of small pits or fenestrations are sometimes visible. The funnel-shaped membranous process above noticed is continuous with the lining membrane. The cavity of each follicle, therefore, communicates with the exterior through the centre of this process, and the aperture is thus guarded by a kind of circular valve permitting the escape of secreted matters, but effectually preventing the entrance of fluid from without.

Some considerations are next offered in support of the view adopted as to the functions of these vascular appendages.

Lastly, on the question whether the peculiarities of structure recognized respectively in *N. Pompilius* and *N. umbilicatus* are sufficient to establish a difference of species, or are attributable merely to variety, the author observes, that any tendency in a being to revert to an original type, when such has been determined, betrays variety; but this tendency is never manifested in the *Nautili* under consideration by the occasional occurrence of specimens presenting characters which place them intermediately between *N. Pompilius* and *N. umbilicatus*. Having visited the Fijii Islands since he formerly wrote on *N. Pompilius*, he finds that the umbilicated *Nautili* are not known to the natives, although *N. Pompilius* is very plentiful; but at Fatuna or Wallis's Island, where both are found, the people recognize the difference between them depending on the presence or absence of umbilical pits. On this the author remarks, that although particular localities, with all attending circumstances, may favour the production of varieties, yet the permanence of the distinctive characters of these *Nautili* without symptom of amalgamation, and the discovery of a female specimen of *N. umbilicatus*, are strong arguments in support of the view that they are distinct species, though very closely allied.

Further descriptive details are given in the explanation of the figures which accompany the memoir.

III. "On a Class of Differential Equations, including those which occur in Dynamical Problems."—Part II. By W. F. Donkin, M.A., F.R.S., F.R.A.S., Savilian Professor of Astronomy in the University of Oxford. Received February 17, 1855.

This is the second and concluding part of a paper of which the first part was printed in the Philosophical Transactions for 1854. In the fourth section (the first of this part) some of the most important results of the former part are recapitulated.

In the fifth section the theory of the Variation of Elements is considered under that aspect which belongs to it in connexion with the general methods of this paper; and the facility of its application is shown in two instances: (1) the expressions for the variations of

the elliptic elements of a disturbed planet's orbit are deduced from the results of Art. 30 (Part I.), on undisturbed elliptic motion; (2) the problem of determining the motion of a free simple pendulum (omitting the effect of the earth's rotation) is treated by considering the orbit of the projection of the bob upon a horizontal plane as a disturbed ellipse. The differential equations which define the variations of the elements of the ellipse are given in a rigorous form, and integrated approximately so as to give the motion of the apsides of the mean ellipse in any case where the pendulum never deviates much from the vertical, and the motion is not very nearly circular. The result agrees with the conclusions of the Astronomer Royal (Proceedings of the Royal Astronomical Society, vol. xi. p. 160).

In the fifth section the transformation of the differential equations by the substitution of new variables is considered, and particularly that kind of transformation, called by the author a *normal transformation*, which leads to a new system of equations, not merely possessing the same general form as the old, but distinguished also by other common properties. A definition is given of those transformations which may be properly called, from analogy, *transformations of coordinates*, and it is shown that all transformations of coordinates are *normal*. General formulæ are given for transforming the equations of any dynamical problem from *fixed* or *moving* systems of axes of coordinates; and an illustration is drawn from the case of the motion of a planet referred to axes in the varying plane of its own orbit.

In the seventh and last section the principles of transformation developed in the preceding section are applied in a more general manner to the differential equations of the planetary theory; and it is shown that when the motions of a planetary system are referred to a system of rectangular axes having their origin in the sun, and otherwise moving in any arbitrary manner, the variations of the elements will still be determined by the same formulæ as if the axes were fixed, provided there be added to the disturbing function R, for each planet, the expression

$$\sqrt{\mu a(1-e^2)}\,.\,(\omega_0 \sin \nu \sin \iota - \omega_1 \cos \nu \sin \iota + \omega_2 \cos \iota),$$

in which ι is the inclination of the orbit to the (moving) plane of xy, the longitude of the node reckoned from the axis of x, and ω_0, ω_1, ω_2;

the angular velocities of the moving system of axes about the three axes themselves. In this expression $\omega_0, \omega_1, \omega_2$ may be any arbitrary functions either of the time or of the elements; but in any case these functions are to be exempted from differentiation with respect to the elements in taking the partial differential coefficients of the disturbing function.

This result is illustrated by referring the motion of a system consisting of *two* planets to axes so chosen that the plane of xy shall always coincide with the *principal plane* of the system, and the axis of x, from which all longitudes are reckoned, shall always coincide with the line of nodes; there are thus obtained twelve rigorous simultaneous differential equations, of which nine form a system apart, containing only the major axes, excentricities, epochs, longitudes of perihelia, and mutual inclination of the two orbits, and afford an example of the so-called "elimination of the nodes;" whilst the remaining three (which contain also these nine elements, but not their differential coefficients) determine the motion of the principal plane and of the line of nodes, relatively to fixed space. The mutual inclination of the two orbits being supposed known, their several inclinations to the principal plane are given by simple relations; and the positions of the planets in their orbits (their longitudes reckoned from the line of nodes) being supposed known, their motions relatively to fixed space would thus be completely determined.

IV. Extract of a Letter, dated January 6, 1855, from J. Mitchell, Esq., Quartermaster of Artillery, Bangalore, "On the Influence of Local Altitude on the Burning of the Fuses of Shells."

"In the early part of the year 1848, at the annual practice of the Artillery in this garrison, it was observed that the fuses burned too long a time. The regular burning of fuses being a matter of much importance, the circumstance was duly reported to Artillery Head Quarters, and a portion of each kind was directed to be sent to St. Thomas's Mount (eight miles from Madras and on the same

level) for examination, where they were found to burn correctly, and as at that time no one suspected the real cause of the discrepance, it was concluded there had been some error in the length of our pendulum, and there for the time the matter rested.

"But as I was satisfied, for many reasons, that it was not owing to an error in the pendulum, I determined to keep the subject in mind, and for the practice in the following year (1849) I caused to be made an adjustable pendulum which beat seconds very correctly for several minutes. This pendulum was daily compared with two or more seconds' watches to make all safe, and, as our longest time of flight did not exceed twenty seconds, no error could possibly arise from this source. The result was that the fuses again burnt too long at Bangalore, and were again found to burn correctly at St. Thomas's Mount.

"This was a mystery to all; but after the matte rhad cost me much thought, it occurred to me that the cause was to be sought in the difference of altitude between St. Thomas's Mount and Bangalore, nearly 3000 feet; and as a means of putting this to the test, I suggested that some fuses should be burnt at the Mount, or at Madras, under a receiver, exhausted until the barometer stood at the Bangalore mean height, or about 27 inches. This, however, it was not found convenient to do; but, as an equally satisfactory way of testing the accuracy of my conclusions, a small number of fuses were prepared and burnt at St. Thomas's Mount, at Bangalore, and at two different altitudes on the Neilgherry Hills, as will be seen by the annexed copy of an official memorandum; and although this experiment was too limited to enable us to compile a scale of the probable times a certain length of fuse composition would burn at given altitudes, it amply proves the fact that combustion is retarded at considerable elevations.

"Memorandum of an experiment to ascertain whether the atmosphere influences the burning of fuses:—

"Eighteen 8-inch fuses, made of the same description of wood (Congo), were filled with composition made for the purpose. The same man drove the whole on the morning of the 2nd August 1849, using the same mallet and drifts. Six of the fuses were burnt at the Mount, six at Bangalore, and six on the Neilgherry Hills; all in the presence of artillery officers; the result is shown below":—

Time.	Station.	Length of fuse.	Time of burning.	Barometer.	Thermometer.	Height above the level of the sea.
		in.	secs.	in.		
August 2, 1849. ¼ past 12 o'clock.	St. Thomas's Mount.	3	14·15	29·78	89°	Artillery Depôt yard.
		3	14·30			
		3	14·30			
		3	14·30			
		3	14·15			
		3	14·30			
August 17, 1849. ½ past 5 P.M.	Bangalore.	3	16·00	26·89	82	3000 ft.
		3	0·0*			
		3	15·75			
		3	15·15			
		3	15·75			
		3	16·25			
August 31, 1849. ½ past 7 A.M.	Kotagherry, Neilgherries.	3	17·00	24·025	Att. 62·7 Det. 61·8	6500 ft.
		3	17·00			
		3	0·0*			
		3	17·30			
Sept. 8, 1849. 20 minutes past 8 A.M.	Cotacamund, Neilgherries.	3	18·00	23·030	54·2	7300 ft.
		3	18·25			
		3				

The writer attributes the result to the rarity of the atmospheric air, and of its constituent oxygen at the higher stations.

* The fuses thus marked accidentally ignited at both ends.

March 29, 1855.

THOMAS BELL, Esq., V.P., in the Chair.

The following communications were read:—

I. "On the existence of an element of Strength in beams subjected to Transverse Strain, arising from the Lateral Action of the fibres or particles on each other, and named by the author the 'Resistance to Flexure.'" By WILLIAM HENRY BARLOW, Esq., C.E., F.R.S. Derby. Received February 23, 1855.

The author commences by observing, that under the existing theory of beams, which recognizes only two elements of resistance, namely tension and compression, the strength of a beam of cast iron cannot be reconciled with the results of experiments on the direct tensile strength, if the neutral axis is in the centre of the beam.

He then proceeds to describe experiments made on two solid beams of cast iron to determine the position of the neutral axis. The beams employed were 7 feet long, 6 inches deep and 2 inches thick, on each of which small vertical ribs were cast, 12 inches apart; nine small holes were drilled opposite to each other in each rib, for the purpose of inserting the pins of a delicate measuring instrument. The distances of the holes of the centre division of both beams were measured under various strains, and the results show that the extensions and compressions proceed in an arithmetical ratio from the centre to the upper and lower sides of the beam; and that at any given distance on either side of the centre, the amount of extension is equal to the amount of compression.

The position of the neutral axis being thus conclusively ascertained to be in the centre, it is shown that, not only the ultimate strength,

but also the amount of extension and compression with a given strain, indicates the existence of another element of resistance, in addition to the resistances to extension and compression.

The author then points out, that in applying the law of "*ut tensio sic vis*" to contiguous fibres, under different degrees of extension and compression, the effect of the lateral adhesion has been omitted, and each fibre has been supposed to be capable of taking up the same degree of extension or compression as if it acted separately, and independently of the adjoining fibres.

It is then shown that this supposed independent action of the fibres is inconsistent with other practical results, and evidence is exhibited of a powerful lateral action when unequal strains are exerted.

From these and other considerations, the author is led to think that the effect of the lateral action, tending to modify the effect of the unequal and opposite strains in a beam, constitutes, in effect, a "resistance to flexure" acting in addition to the resistances of tension and compression.

In order to ascertain whether the apparent difference in the amount of tensile strength when excited by direct and transverse strains is due to *flexure*, the author caused open beams or girders to be made, each of which was formed by two bars of metal; the upper and lower bars of the same beam were in every case of the same form and dimensions; but the depth of metal and the distance to which the bars were separated vertically, was varied in the several forms of girder experimented upon. By these means the bar forming the lower side of each girder was torn asunder under different degrees of flexure. The different forms of girder experimented upon were of equal length, and were compared with solid beams and with bars of the same metal broken by direct tensile strain.

From the mean of *four* experiments on each form of girder, the value of the total resistance at the outer fibre is ascertained, and exhibits the following results:—

1. In girders having the same depth of metal, namely about 2 inches, but the total depth of the girder, and consequently the deflections different—

Form of beam or girder.	Depth of girder.	Deflection.	Total resistance at the outer fibre.
			lbs.
No. 1. Solid beam	2·02	·670	41709
No. 2. Open girder	2·51	·510	35386
No. 3. Open girder	3·00	·401	31977
No. 4. Open girder	4·00	·301	28032

2. In girders having the same total depth (namely 4 inches), and consequently nearly the same deflection, but differing in the depth of metal—

Form of beam or girder.	Depth of metal.	Total resistance at the outer fibre.
		lbs.
No. 5. Open girder	3·01	37408
No. 4. Open girder	1·97	28032
No. 7. Open girder	1·56	27908
No. 6. Open girder	1·48	25271
The tensile strength of the metal employed was found to be		18750

From these experiments, the particulars of which are fully detailed, the following facts are elicited :—

1. That in all cases the total resistance at the outer fibre, at the time of rupture, is greater than the tensile strength.

2. That in girders having the same depth of metal, it increases when the deflection increases ; and

3. That in girders having the same total depth, and the same deflection, the resistance is greater when the depth of metal in the beam is greater.

And it follows that there is an element of strength depending on the depth of metal in connexion with the deflection ; or in other words, dependent on the *degree* of *flexure* to which the metal forming the beam is subjected.

The author next proceeds to examine the law under which this resistance varies ; and considering the total resistance in the solid beam to be composed of two resistances, one being constant and due to the tensile strength, and the other variable and depending on the depth of the metal in connexion with its deflection, the experiments indicate that the resistance to flexure varies, throughout all

the girders, directly as the amount of deflection into the depth of the metal.

The paper concludes by pointing out the important amount of this resistance, the operation of which has been hitherto unknown, and which in cast iron exceeds the tensile strength of the metal, and shows that comparisons of the strength of different forms of section, based on the existing theory, which assumes the resistance at the outer fibre to be constant and equal to the tensile strength of the metal, must be entirely fallacious.

The paper is accompanied by full details of all the experiments, and the measurements for determining the position of the neutral axis.

II. "On the Metallic and some other Oxides, in relation to Catalytic Phenomena." By the Rev. J. Eyre Ashby. Communicated by the Rev. John Barlow, F.R.S. Received March 8, 1855.

I purpose to detail some experiments on the metallic (and a few other) oxides, made with a view to ascertain their powers to produce and maintain, catalytically, the combustion of various gases and vapours; and to annex such considerations as appear to be suggested by the facts. By catalysis I understand the operation of one body upon another, under favourable circumstances, whereby the second body is resolved into new chemical combinations, while the first (whatever may happen during the process) remains finally unchanged. This must be taken as not including explosion by percussion, in which the change takes place owing to the external application of dynamic force.

The apparatus for experimenting comprehends a variety of shallow capsules; wire-gauze, of iron, copper, and brass, of different degrees of fineness, cut into discs a little larger than the vessels on which they are to be superimposed; a spirit-lamp with large wick; a pair of pliers, and a few rings of wire to support the gauze, if necessary, while heating it in the spirit flame. The method of procedure is simple: the watch-glass, or capsule, is *nearly* filled with

the liquid whose vapour is to be tried; on a wire-gauze disc is spread the oxide whose powers as a catalyser are to be tested, and this being warmed (more or less) over the lamp, is set down upon the upper rim of the capsule. Sometimes it is necessary to heat a layer of the oxide in the middle of a small combustion-tube, and pass over it the gas, or mixture of gases.

I tried the following substances with pyroxylic spirit (hydrated oxide of methyl) and alcohol separately.

1. $Co.O$ appeared to possess the power in some degree, but perhaps the specimen was in too dense agglomeration, which is not essentially reduced by trituration.

2. Co_2O_3 maintained the catalytic combustion very well.

3. $Ag.O$, reduced to metallic silver, which shows a strong tendency on gauze, and acts perfectly in the combustion-tube.

4. U_2O_3, HO became, at red heat, anhydrous mixture of $U\,O$ and U_3O_4, showing strong tendency. A very pure specimen catalysed the vapour as it changed from yellow to green, after which it died away. Will not act below 570° (F.).

5. $Sn.O$; strong tendency.

6. $Sn.O_2$; slight tendency.

7. $W.O_3$ apparently produces the effect if placed while glowing, over alcohol, but gradually dies away, as if very slowly cooling.

8. Pb_3O_4 changed to $Pb.O$, and showed a strong tendency, but quickly faded and grew cold.

9. Cd, O, placed while very warm over pyroxylic spirit, burst into glow and catalysed, but always died off after the lapse of from half a minute to two or three minutes, and then became incapable.

10. $Ca.O$ (on the gauze), no effect.

11. $Si.O_2$ exhibited a tendency.

12. Stourbridge clay; no effect.

13. Al_2O_3 appeared to have no effect in maintaining catalytic combustion on the gauze, but when made red-hot and quenched in absolute alcohol, it changed from pure white to a black substance and oxidized a portion of the alcohol. That this is not owing to carbon in the alcohol is evident, because the same change occurs when it is quenched in strong liquid ammonia. I suspect that it is a new oxide of aluminium.

14. Ni_2O_3, formed by heating carbonate of nickel nearly to red-

ness, failed; prepared from the common nitrate, it acted for a short time; reduced as an intensely black velvety substance from the purest nitrate, then warmed but not made red-hot, it glowed and catalysed with alcohol or ether. With pyroxylic spirit, it was left at the end of the operation of a greenish drab, which I suspect to be a mixture of $Ni_2 O_3$ with $Ni.O$, although it may be $Ni_2 O_3$ changed only in appearance, for when treated with nitric acid no nickel is dissolved.

15. $Mn O_2$ is changed at red heat into $Mn_2 O_3$, which, with alcohol, ether, and pyroxylic spirit, continues the slow combustion very steadily. A specimen of very pure $Mn_2 O_3$ acted extremely well, as did also a portion of "euchrome" (a hydrated sesquioxide of manganese (impure) dug from the estate of Lord Audley), after being heated in the air to drive away the carbonaceous matter with which it was mingled. $Mn_2 O_3$ will, if sufficient care be taken, catalyse the moist gas arising from a strong solution of ammonia.

16. $Fe_2 O_3$, when in the state of a light puffy powder, catalyses the vapour of ether, alcohol, and pyroxylic spirit, only requiring to be heated on the gauze before it is laid over the capsule. It is cheap, easily employed, and of invariable action. I have kept up the combustion for several hours on a surface of 120 square inches.

By means of a catalytic lamp in which the liquid employed is continually supplied from a reservoir and maintained at a constant level in the capsule, I have used 7 or 8 square inches continuously during thirty-six hours. This lamp I have occasionally used for laboratory purposes, where a gentle and equable heat was required for several hours.

Pursuing my experiments with the oxides of the metals, heated on wire gauze, I tried as many as I could procure or make, and by a tolerably wide induction I found that the *sesquioxides* have the strongest tendency to produce and maintain the catalytic glow, and do produce it in every case in which they are not decomposed by the amount of heat required to begin the operation.

When hydrated $Fe_2 O_3$ is heated and placed over alcohol, its colour is deepened towards black, but not uniformly, and when cold the original colour returns. But if it be made red-hot and quenched in boiling alcohol out of contact with air, it is converted into hydrated $Fe_3 O_4$, and remains permanently a deep black magnetic powder, soluble in acids. A strong solution of ammonia may be substituted

for the alcohol with the same effect, but in this case some of the sesquioxide will remain unaltered and mixed with the black oxide. The alcohol or ammonia is correspondingly changed by oxidation derived from the oxygen which has been released from combination with the iron. If the hydrated $Fe_3 O_4$ be heated in contact with air, it immediately (even when it has been kept for many months) becomes $Fe_2 O_3$ by oxidation from the atmosphere, but if heated to redness *in vacuo*, it cools unchanged. [Can the black powder of alumina be $Al_3 O_4$, formed in a similar way?] The process of catalysation by $Fe_2 O_3$ is thus evident; the heated sesquioxide loses a portion of oxygen to the alcohol and becomes $Fe_3 O_4$, which is instantly reconverted into $Fe_2 O_3$ by receiving oxygen from the air, and this alternation is constantly going on in every portion of the glowing mass. It is not a mere *action de présence*, but alternate reduction and oxidation of the sesquioxide, producing a continuous oxidation of the alcohol.

This suggests a consideration apparently adverse to the atomic hypothesis of Dr. Dalton. How can a single compound molecule $Fe_2 O_3$ be changed by deoxidation into another compound molecule $Fe_3 O_4$, when, according to theory, there are in it but *two* combining proportions of iron, whereas the resultant contains *three*? and how (by deoxidation) can the resultant molecule contain *four* combining proportions of oxygen when the primary contained only *three*? We can indeed represent to the imagination that *three* molecules of the sesquioxide, acting as if they were one triple molecule, lose *one* combining proportion of oxygen, and are converted into *two* molecules of the black oxide; and conversely, that *two* molecules of the black oxide, acting as if they were *one* double molecule, combine with *one* atom of oxygen, and are converted into *three* atoms of sesquioxide. The only way to account for this, in accordance with the popular atomic theory, seems to be, to assume that the notation for these oxides is incorrect, and that

for $Fe_2 O_3$ we should write $Fe_6 O_9$,
and for $Fe_3 O_4$ we should write $Fe_6 O_8$.

If the current notation be retained, and any law be admitted, in virtue of which three molecules of sesquioxide may suffer reduction as if they were only one molecule, and divide into two molecules of the magnetic oxide, we might conceive a peculiar structure in the

$Fe_3 O_4$, with a tendency to separate again into $FeO, Fe_2 O_3$; that it is really in combination as $Fe_3 O_4$, but ready to yield to slight causes and become $FeO, Fe_2 O_3$. This would explain Mr. Mercer's experiment (quoted by Brande, I. 716, edition 1848) of the chemical union of a mechanical mixture of protoxide and peroxide of iron.

Perhaps the sesquioxides occupy a middle place in the scale of effects. Take the case of iron; we have

$Fe + \mathbf{O}$, pyrophorus,—violent oxidation,
$Fe_2 O_3 + \mathbf{NH}_3 + \mathbf{O}$, alternate reduction and oxidation,
$Fe\, O_3 + \mathbf{NH}_3$ (**in water**), reduction.

To show the last, add ammonia to a solution of FeO_3, KO, and $Fe_2 O_3$ will be precipitated.

A mixture of ten parts by weight of powdered chlorate of potassa with one part of $Fe_2 O_3$ disengages oxygen with extreme facility and great economy of heat as compared with the oxides of copper and manganese; and it is the more convenient because *n* grains of the mixture will represent almost exactly *n* cubic inches of disengaged oxygen.

A state of mechanical division is not absolutely necessary for the catalysation of some inflammable vapours by $Fe_2 O_3$; an old nail, entirely transmuted into rust, will perform the operation; and when we consider that in many cases of fermentation, decay and putrefaction, this oxide may be present, divided or aggregated, while heat is evolved, and inflammable gases and vapours are set free, we may hereafter be able to trace some instances of "spontaneous combustion" to the catalytic action of the sesquioxides of iron.

III. "Ocular Spectres and Structures as Mutual Exponents." By James Jago, A.B. Cantab., M.B. Oxon., Physician to the Royal Cornwall Infirmary. Communicated by Professor Stokes, Sec. R.S. Received March 5, 1855.

The present communication is a revised and modified version of a paper bearing the same title, which was read on the 18th of January and 1st of February, and which was, by permission, withdrawn. The chief modification applies to the author's views respecting the struc-

ture of the vitreous body as deduced from entoptical phenomena. He is now of opinion that the arborescent system of which he infers the existence in that part of the eye (Proceedings, p. 209) does not consist of *tubes* filled with globules or cells, as he at first supposed, but of cell-constituted filaments. In a note, dated March 27, 1855, he gives the following enunciation of his present views as to the structure of the vitreous body :—

"In the vitreous humour are innumerable transparent globules, beads or cells, of less specific gravity than the fluid, extremely minute, and of uniform size, which are arranged, *without exception*, in rows to form the threads of a lax, elongated, irregular web, springing from the general surface of the capsule by, commonly, exquisitely small meshes, and extending into the interior by others of increasing size, so that the innermost part of the web—which lies nearer the circumference of the vitreous body than its centre—consists of comparatively large ones.

"Whenever the eye rotates, this filamentous peripheral system will in its relative (counter) rotation, *gradually, though soon,* come to the end of its tether, and in this interval and when there, act as a check upon the relatively rotating fluid; perpetually reiterating obstruction, above all in the immediate vicinity of the capsule and (by the disposition of the threads to float vertically) most effectually in the most important or horizontal direction. The middle of the vitreous body being free of impediment, relative rotation of the fluid expends itself there, whilst a practical concurrence in the ocular rotation ensues near the capsule. And thus in the incessant movements of the eye, head and body, the wall that confines the fluid can suffer no severe concussions from eddies in the latter. In other words, we have herein a provision that the crystalline lens may not be shaken, the circulation in the retinal vessels may not be deranged, and sensations of light may not be ever assailing us from impulses of the vitreous fluid, perhaps that the retina may not itself suffer direct injury therefrom."

IV. "An Account of some Experiments made with the Submarine Cable of the Mediterranean Electric Telegraph." By CHARLES WHEATSTONE, F.R.S. Received March 29, 1855.

The following results were obtained between May 24 and June 8 in last year, with the telegraphic cable manufactured by Messrs. Kuper and Co. of East Greenwich, for the purpose of being laid across the Mediterranean sea, from Spezia on the coast of Italy to the island of Corsica. The manufacturers, in conjunction with Mr. Thomson the engineer of the undertaking, kindly afforded me every facility in carrying on the experiments. The short time that elapsed between the opportunity presenting itself and the shipping of the cable for its destination, prevented me from determining with sufficient accuracy some points of importance, respecting which I was only able to make preliminary experiments, but the following, which I was able to effect with the means at hand, may possess sufficient interest to be made public. They present perhaps nothing theoretically new, but I am not aware that experimental verifications of some of these points have been made before. I assume that the reader is acquainted with the experiments of Dr. Faraday described in the Philosophical Magazine, N. S., vol. vii. p. 197.

The cable was 110 miles in length, and contained six copper wires, one-sixteenth of an inch in diameter, each separately insulated in a covering of gutta percha one-tenth of an inch in thickness. The whole was surrounded by twelve thick iron wires twisted spirally around it, forming a complete metallic envelope one-third of an inch in thickness. A section of the cable presented the six wires arranged in a circle of half an inch diameter, and one-fifth of an inch from the internal surface of the iron envelope.

The cable was coiled in a dry well in the yard, and one of its ends was brought into the manufactory. The wires were numbered 1, 2, 3, 4, 5, 6, and the ends in the well were indicated by an accent; the ends 1′2, 2′3, 3′4, 4′5, 5′6 were connected by supplementary wires, so that the electric current might be passed in the same direction through all the six wires joined to a single length, or through

any lesser number of them, the connexions being made at pleasure in the experimenting room.

The rheomotor employed was an insulated voltaic battery consisting of twelve troughs, each of twelve elements, which had been several weeks in action.

First Series.

The following experiments show that the iron envelope of the compound conductor gives rise to the same phenomena of induction which occur when the insulated wire is immersed in water, as in Dr. Faraday's experiments.

Exp. 1. One end of the entire length, 660 miles, was brought in connexion with one of the poles of the battery, the other end remaining insulated. The wire became charged with negative electricity when its end touched the zinc pole, and with positive electricity when it communicated with the copper pole. A current, indicated by a galvanometer placed near the battery, existed as long as the charge was going on, and ceased when it arrived at its maximum. [The feeble current attributed to imperfect insulation, which continues as long as the contact with the battery remains, is here left out of consideration.] When the wire was charged, and the discharge effected by a wire communicating with the earth, the current produced was in the same direction, whether the discharge was made near the battery or at the opposite end; *i. e.* the current in both cases proceeded from the wire to the earth in the same direction.

Exp. 2. On bringing one end of the wire in contact with one of the poles of the battery, the other pole having no communication with the earth, the wire remained uncharged. A very slight and scarcely perceptible tremor was observed in the galvanometer needle interposed between the battery and the wire.

Exp. 3. To each of the poles of the battery was attached a wire 220 miles in length, and similar galvanometers were interposed between the two wires (the remote extremities of which remained insulated) and the battery. So long as one wire alone was connected with the battery no charge was communicated to it, but on connecting the other wire with the opposite pole both wires were instantaneously charged, as the strong deflection of both needles rendered evident. On bringing the free end of one of the wires in communi-

cation with the earth it alone was discharged, the other wire remaining fully charged.

Second Series.

Exp. 4. One pole of the battery was connected with the earth, and the other with 660 miles of wire, which had an earth communication at its opposite end; three galvanometers were interposed in the course of the conductor; the first near the battery, the second in the middle of the wire, *i. e.* 330 miles from each extremity, and the third at the remote end near the communication with the earth. When the connexion of the battery with the wire was completed, the galvanometers were successively acted upon in the order of their distances from the battery, as in the experiments recorded by Dr. Faraday. When the earth connexion at the remote extremity of the wire, on the contrary, was completed, the disturbance of equilibrium commenced at this end, and the galvanometers successively acted in the reverse order, *i. e.* the galvanometer which was the most distant from the battery was the first impelled into motion. In the latter case, before the completion of the circuit the needles of the galvanometers had assumed constant deflections to a limited extent, owing to a feeble current arising from the uniform dispersion of the static electricity along the wire.

Exp. 5. The two extremities of the 660 miles of wire were brought into connexion with the opposite poles of the battery. When one of the ends previously disconnected from the battery was united therewith, the galvanometers at the extremities of the wire, and consequently which were at equal distances from the poles of the battery, were immediately and simultaneously acted upon, while that which was in the middle of the wire was subsequently caused to move. When the wire disconnected in the middle instead of near one of the poles of the battery was again united, the middle galvanometer, which was the most remote from the battery, was the first acted upon, and those near the poles subsequently.

The comparison of the two above-mentioned experiments show that the earth must not be regarded simply as a conductor, which many suppose to be the case. Since in the first experiment there were not many yards' distance between the two earth terminations, did the extent of ground between them act only as a conductor, the two

galvanometers at the extremities of the wire should have acted simultaneously, as in the second experiment, and as would have been the case had a short wire united the two extremities which proceeded to the earth.

Third Series.

Exp. 6. One pole of the battery was connected with the earth, and the opposite pole with one extremity of the 660 miles of wire, the other end remaining insulated; a delicate galvanometer was interposed near the battery. Notwithstanding there was no circuit formed, the needle showed a constant deflection of $33\frac{1}{2}°$; the feeble current thus rendered evident is not so much to be attributed to imperfect insulation, as to the uniform and continual dispersion of the static electricity with which the wire is charged throughout its entire length, in the same manner as would take place in any other charged body placed in an insulating medium. The strength of the current thus occasioned appears to be nearly, if not exactly, proportional to the length of the wire added, as the following table will show: the first column indicates the number of miles of wire subjoined beyond the galvanometer, and the second the corresponding deflections of the needle:—

miles.	°
0	0
110	$6\frac{1}{2}$
220	12
330	18
440	$23\frac{1}{2}$
550	28
660	31

Exp. 7. One end of the 660 miles of wire was now allowed to remain constantly in contact with one of the poles of the battery; but the galvanometer was successively shifted to different distances from the battery. The strength of the current was now shown to be inversely as the distance of the galvanometer from the battery, becoming null at its extremity, as shown in the following table. The first column shows the distance from the battery at which the galvanometer was placed, and the second column the corresponding deflection of the needle.

miles.	°
Near the battery	$33\frac{1}{2}$
110	31
220	25
330	15
440	12
550	5
660	0

The deflections of the needle of the galvanometer employed in these experiments were, when they did not surpass 36°, very nearly comparable with the force of the current. This I ascertained in the following way. I took six cells of the small constant battery described in my paper " On new Instruments and Processes for determining the Constants of a Voltaic Circuit," printed in the Philosophical Transactions for 1843, and placed in the circuit formed of the 660 miles of wire, the earth, and the galvanometer, successively 1, 2, 3, 4, 5 and 6 cells. Leaving out of consideration the resistances in the cells themselves and in the earth, which were very inconsiderable in comparison with that in the long wire, the force of the current should be approximately proportionate to the number of the elements; and since the deflections of the needle nearly indicated this proportionality, as the following table will show, it may be assumed that the force of the current, when the deflection of the needle did not surpass 36°, nearly corresponded with the angular deviation.

cell.	°
1	6
2	14
3	19
4	28
5	32
6	36

From the preceding experiments (6. and 7.) it seems to result, that whatever length of wire is connected with the battery, if a galvanometer is placed at the farther extremity of the wire and a constant length added to the other termination of the galvanometer, its indication remains always nearly the same. Thus the galvanometer indicated $6\frac{1}{2}$° when it was placed close to the battery and 110 miles

of wire were subjoined beyond it; and 5° when 550 miles were interposed between the battery and galvanometer, the same length, 110 miles, being subjoined. In like manner, when 220 miles were added beyond the galvanometer placed near the battery, the indication was 12°; precisely the same as when 440 miles were interposed and 220 added. So also when 330 miles were added, the deviation of the galvanometer was 18°; and 15° when 330 miles were interposed and 330 added. I have no doubt that the correspondence would have been closer had it not been for the fluctuations of the battery.

It would appear from this, that whatever be the length of wire attached to the insulated pole of a battery, it becomes charged to the same degree of tension throughout its entire extent; so that another insulated wire brought into connexion with its free extremity exhibits precisely the same phenomena, in kind and measure, as when it is brought into immediate connexion with a pole of the battery. Some important practical consequences flow from this conclusion, which I will not develope at present, as I have not yet had an opportunity of submitting them to the test of experiment.

April 19, 1855.

The LORD WROTTESLEY, President, in the Chair.

The Right Hon. Lord Hatherton was admitted into the Society.

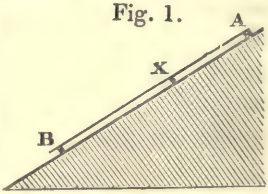
The following communications were read:—

I. "On the descent of Glaciers." By the Rev. HENRY MOSELEY, M.A., F.R.S., Corresponding Member of the Institute of France. Received March 15, 1855.

If we conceive two bodies of the same form and dimensions (cubes for instance), and of the same material, to be placed upon a uniform

horizontal plane, and connected by a substance which alternately extends and contracts itself, as does a metallic rod when subjected to variations of temperature, it is evident that by the extension of the intervening rod each will be made to recede from the other by the same distance, and, by its contraction, to approach it by the same distance. But if they be placed on an inclined plane (one being lower than the other), then when by the increased temperature of the rod its tendency to extend becomes sufficient to push the lower of the two bodies downwards, it will not have become sufficient to push the higher upwards. The effect of its extension will therefore be to cause the lower of the two bodies to descend whilst the higher remains at rest. The converse of this will result from contraction; for when the contractile force becomes sufficient to pull the upper body down the plane it will not have become sufficient to pull the lower up it. Thus, in the contraction of the substance which intervenes between the two bodies, the lower will remain at rest whilst the upper descends. As often, then, as the expansion and contraction is repeated the two bodies will descend the plane until, step by step, they reach the bottom.

Fig. 1.



Suppose the uniform bar AB placed on an inclined plane, and subject to extension from increase of temperature, a portion XB will descend, and the rest XA will ascend; the point X where they separate being determined by the condition that the force requisite to push XA up the plane is equal to that required to push XB down it.

Let AX$=x$, AB$=$L, weight of each linear unit $=u$, $i=$ inclination of plane, $\phi=$ limiting angle of resistance,

$$\therefore \quad ux = \text{weight of AX},$$

$$u(\mathrm{L}-x) = \text{weight of BX}.$$

Now, the force acting parallel to an inclined plane which is necessary to push a weight W up it, is represented by

$$\mathrm{W}\frac{\sin(\phi+i)}{\cos\phi};$$

and that necessary to push it down the plane by

$$\mathrm{W}\frac{\sin(\phi-i)}{\cos\phi};$$

$$\therefore \quad ux\frac{\sin(\phi+i)}{\cos\phi}=u(\mathrm{L}-ux)\frac{\sin(\phi-i)}{\cos\phi}$$

$$\therefore \quad x\{\sin(\phi+i)+\sin(\phi-i)\}=\mathrm{L}\sin(\phi-i)$$

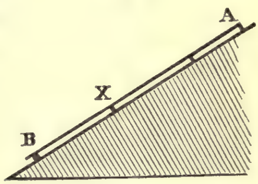
$$\therefore \quad 2x\sin\phi\cos i=\mathrm{L}\sin(\phi-i)$$

$$\therefore \quad x=\frac{1}{2}\mathrm{L}\frac{\sin(\phi-i)}{\sin\phi\cos i}$$

$$\therefore \quad x=\frac{1}{2}\mathrm{L}\left\{1-\frac{\tan i}{\tan\phi}\right\}.$$

When contraction takes place the converse of the above will be true. The separating point X will be such, that the force requisite to pull XB up the plane is equal to that required to pull AX down it. BX is obviously in this case equal to AX in the other.

Fig. 2.



Let λ be the elongation per linear unit under any variation of temperature; then the distance which the point B (see fig. 1) will be made to descend by this elongation

$$=\lambda.\overline{\mathrm{BX}}$$
$$=\lambda(\mathrm{L}-x)$$
$$=\frac{1}{2}\lambda\mathrm{L}\left(1+\frac{\tan i}{\tan\phi}\right).$$

If we conceive the bar now to return to its former temperature, contracting by the same amount (λ) per linear unit; then the point B (fig. 2) will by this contraction be made to ascend through the space

$$\mathrm{BX}.\lambda=x\lambda$$
$$=\frac{1}{2}\mathrm{L}\lambda\left\{1-\frac{\tan i}{\tan\phi}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

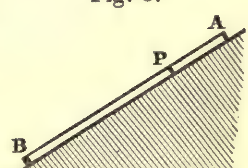
Total descent l of B by elongation and contraction is therefore determined by the equation

$$l=\mathrm{L}\lambda\frac{\tan i}{\tan\phi}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

To determine the pressure upon a nail, driven through the rod at any point P fastening it to the plane.

It is evident that in the act of extension the part BP of the rod will descend the plane and the part AP ascend; and

Fig. 3.



conversely in the act of contraction; and that in the former case the nail B will sustain a pressure upwards equal to that necessary to cause BP to descend, and a pressure downwards equal to that necessary to cause PA to ascend; so that, assuming the pressure to be downwards, and adopting the same notation as before, except that AP is represented by ρ, AB by a, and the pressure upon the nail (assumed to be downwards) by P, we have in the case of extension

$$P=u\rho\frac{\sin(\phi+i)}{\cos\phi}-u(a-\rho)\frac{\sin(\phi-i)}{\cos\phi},$$

and in the case of contraction

$$P=u(a-\rho)\frac{\sin(\phi+i)}{\cos\phi}-u\rho\frac{\sin(\phi-i)}{\cos\phi}.$$

Reducing, these formulæ become respectively

$$P=\frac{u}{\cos\phi}\left\{2\rho\sin\phi\cos i-a\sin(\phi-i)\right\} \quad . \quad . \quad . \quad (3)$$

$$P=\frac{u}{\cos\phi}\left\{a\sin(\phi+i)-2\rho\sin\phi\cos i\right\} \quad . \quad . \quad . \quad (4)$$

My attention was first drawn to the influence of variations in temperature to cause the descent of a lamina of metal resting on an inclined plane, by observing, in the autumn of 1853, that a portion of the lead which covers the south side of the choir of the Bristol Cathedral, which had been renewed in the year 1851, but had not been properly fastened to the ridge beam, had descended bodily 18 inches into the gutter; so that if plates of lead had not been inserted at the top, a strip of the roof of that length would have been left exposed to the weather. The sheet of lead which had so descended measured, from the ridge to the gutter, 19 feet 4 inches and along the ridge 60 feet. The descent had been continually going on, from the time the lead had been laid down. An attempt made to stop it by driving nails through it into the rafters had failed. The force by which the lead had been made to descend, whatever it was, had been found sufficient to draw the nails*. As the pitch of

* The evil was remedied by placing a beam across the rafters near the ridge, and doubling the sheets round it, and fixing their ends with spike nails.

the roof was only $16\frac{1}{2}°$, it was sufficiently evident that the weight of the lead alone could not have caused it to descend. Sheet lead, whose surface is in the state of that used in roofing, will stand firmly upon a surface of planed deal when inclined at an angle of 30°*, if no other force than its weight tends to cause it to descend. The considerations which I have stated in the preceding articles led me to the conclusion that the daily variations in the temperature of the lead, exposed as it was to the action of the sun by its southern aspect, could not but cause it to descend considerably, and the only question which remained on my mind was, whether this descent could be so great as was observed. To determine this I took the following data:—

Mean daily variation of temperature at Bristol in the month of August, assumed to be the same as at Leith (Kaemtz, Meteorology, by Walker, p. 18), 8°·21 Cent.

Linear expansion of lead through 100° Cent. ·0028436.

Length of sheets of lead forming the roof from the ridge to the gutter 232 inches.

Inclination of roof 16° 32′.

Limiting angle of resistance between sheet lead and deal 30°.

Whence the mean daily descent of the lead, in inches, in the month of August, is determined by equation (2) to be

$$l = 232 \times \frac{8°{\cdot}21}{100} \times {\cdot}0028436 \times \frac{\tan 16°\ 32'}{\tan 30°}$$

$$l = {\cdot}027848 \text{ inches.}$$

The average daily descent gives for the whole month of August a descent of ·863288. If the average daily variation of temperature of the month of August had continued throughout the year, the lead would have descended 10·19148 inches every year. And in the

* This may easily be verified. I give it as the result of a rough experiment of my own. I am not acquainted with any experiments on the friction of lead made with sufficient care to be received as authority in this matter. The friction of copper on oak has, however, been determined by General Morin to be 0·62, and its limiting angle of resistance 31° 48′; so that if the roof of Bristol Cathedral had been inclined at 31° instead of 16°, and had been covered with sheets of copper resting on oak boards, instead of sheets of lead resting on deal, the sheeting would not have slipped by its weight only.

two years from 1851 to 1853 it would have descended 20·38296 inches. But the daily variations of atmospheric temperature are less in the other months of the year than in the month of August. For this reason, therefore, the calculation is in excess. For the following reasons it is in defect:—1st, the daily variations in the temperature of the lead cannot but have been greater than those of the surrounding atmosphere. It must have been heated above the surrounding atmosphere by radiation from the sun in the day-time, and cooled below it by radiation into space at night. 2ndly. One variation of temperature only has been assumed to take place every twenty-four hours, viz. that from the extreme heat of the day to the extreme cold of the night; whereas such variations are notoriously of constant occurrence during the twenty-four hours. Each cannot but have caused a corresponding descent of the lead, and their aggregate result cannot but have been greater than if the temperature had passed uniformly (without oscillations backwards and forwards) from one extreme to the other.

These considerations show, I think, that the causes I have assigned are sufficient to account for the fact observed. They suggest, moreover, the possibility that results of importance in meteorology may be obtained from observing with accuracy the descent of a metallic rod thus placed upon an inclined plane. That descent would be a measure of the aggregate of the changes of temperature to which the metal was subjected during the time of observation. As every such change of temperature is associated with a corresponding development of mechanical action under the form of work*, it would be a measure of the aggregate of such changes and of the work so developed during that period; and relations might be found between measurements so taken in different equal periods of times, successive years for instance, tending to the development of new meteorological laws.

The following are the results of recent experiments† on the expansion of ice:—

* Mr. Joule has shown (Phil. Trans. 1850, Part I.) that the quantity of heat capable of raising a pound of water by 1° Fahr. requires for its evolution 772 units of work.

† Vide Archiv f. Wissenchaftl. Kunde v. Russland, Bd. vii. S. 333.

Linear expansion of ice for an interval of 100° of the Centigrade thermometer.

0·00524, Schumacher.
0·00513, Johrt.
0·00518, Moritz.

Ice, therefore, has nearly twice the expansibility of lead, so that a sheet of ice would, under similar circumstances, have descended a plane similarly inclined, twice the distance that the sheet of lead referred to in the preceding article descended. Glaciers are, on an increased scale, sheets of ice placed upon the slopes of mountains, and subjected to atmospheric variations of temperature throughout their masses by variations in the quantity and the temperature of the water, which flowing from the surface everywhere percolates them. That they must from this cause descend into the valleys is therefore certain. That portion of the Mer de Glace of Chamouni which extends from Montanvert to very near the origin of the Glacier de Léchaud, has been accurately observed by Professor James Forbes*. Its length is 22,600 feet, and its inclination varies from 4° 19′ 22″ to 5° 5′ 53″. The Glacier du Géant, from the Tacul to the Col du Géant, Professor Forbes estimates (but not from his own observations, or with the same certainty) to be 24,700 feet in length, and to have a mean inclination of 8° 46′ 40″.

According to the observations of De Saussure, the mean daily range of Reaumur's thermometer in the month of July, at the Col du Géant, is 4°·257†, and at Chamouni 10°·092. The resistance opposed by the rugged channel of a glacier to its descent cannot but be different at different points and in respect to different glaciers. The following passage from Professor Forbes's work contains the most authentic information I am able to find on this subject. Speaking of the Glacier of la Brenva, he says, "The ice removed, a layer of fine mud covered the rock, not composed however alone of the clayey limestone mud, but of sharp sand derived from the granitic moraines of the glacier, and brought down with it from the opposite side of the valley. Upon examining the face of the ice removed from contact with the rock, we found it *set* all over

* Travels through the Alps of Savoy. Edinburgh, 1843.

† Quoted by Professor Forbes, p. 231.

with sharp angular fragments, from the size of grains of sand to that of a cherry, or larger, of the same species of rock, and which were so firmly fixed in the ice as to demonstrate the impossibility of such a surface being forcibly urged forwards without sawing and tearing any comparatively soft body which might be below it. Accordingly, it was not difficult to discover in the limestone the very grooves and scratches which were in the act of being made at the time by the pressure of the ice and its contained fragments of stone." (Alps of the Savoy, pp. 203–4.) It is not difficult from this description to account for the fact that small glaciers are sometimes seen to lie on a slope of 30° (p. 35). The most probable supposition would indeed fix the limiting angle of resistance between the rock and the under surface of the ice, set all over, as it is described to be, with particles of sand and small fragments of stone, at about 30°, that being nearly the slope at which calcareous stones will rest on one another. If we take then 30° to be the limiting angle of resistance between the under surface of the Mer de Glace and the rock on which it rests, and if we assume the same mean daily variation of temperature (4·257 Reaumur, or 5·321 Centigrade) to obtain throughout the length of the Glacier du Géant, which De Saussure observed in July at the Col du Géant; if, further, we take the linear expansion of ice at 100° Centigrade to be that (·00524) which was determined by the experiments of Schumacher; and, lastly, if we assume the Glacier du Géant to descend as it would if its descent were unopposed by its confluence with the Glacier de Léchaud, we shall obtain, by substitution in equation (2) for the mean daily descent of the Glacier du Géant at the Tacul, the formula—

$$l = 24700 \times 5{\cdot}321 \times \frac{{\cdot}00524}{100} \times \frac{\tan 8^\circ 46'}{\tan 30^\circ}$$

$$l = 1{\cdot}8395 \text{ feet.}$$

The actual descent of the glacier in the centre was 1·5 foot. If the Glacier de Léchaud descended at a mean slope of 5°, singly in a sheet of uniform breadth to Montanvert without receiving the tributary glacier of the Talèfre, or uniting with the Glacier du Géant, its diurnal descent would be given by the same formula, and would be found to be ·95487 foot. Reasoning similarly with reference to the Glacier du Géant, supposing it to have continued its course singly from the Col du Géant to Montanvert without confluence with the

Glacier de Léchaud, its length being 40,420 feet, and its mean inclination 6° 53′, its mean diurnal motion l at Montanvert would, by formula (2) have been 2·3564 feet *. The actual mean daily motion of the united glaciers, between the 1st and the 28th July, was Montanvert (Forbes's 'Alps of the Savoy,' p. 140),—

Near the side of the glacier	1·441 foot.
Between the side and the centre ..	1·750 foot.
Near the centre	2·141 feet.

The motion of the Glacier de Léchaut was therefore accelerated by their confluence, and that of the Glacier du Géant retarded. The former is dragged down by the latter.

I have had the less hesitation in offering this solution of the mechanical problem of the motion of glaciers, as those hitherto proposed are confessedly imperfect. That of De Saussure, which attributes the descent of the glacier simply to its weight, is contradicted by the fact that isolated fragments of the glacier stand firmly on the slope on which the whole nevertheless descends; it being obvious that if the parts would remain at rest separately on the bed of the glacier, they would also remain at rest when united.

That of Professor J. Forbes, which supposes a viscous or semifluid structure of the glacier, is not consistent with the fact that no viscosity is to be traced in its parts when separated. They appear as solid fragments, and they cannot acquire in their union properties in this respect which individually they have not.

Lastly, the theory of Charpentier, which attributes the descent of the glacier to the daily congelation of the water which percolates it, and the expansion of its mass consequent thereon, whilst it assigns a cause which, so far as it operates, cannot, as I have shown, but cause the glacier to descend, appears to assign one inadequate to the result; for the congelation of the water which percolates the glacier does not, according to the observations of Professor Forbes †, take place at all in summer more than a few inches from the surface. Nevertheless it is in the summer that the daily motion of the glacier is the greatest.

* On the 1st of July, the centre of the actual motion of the Mer de Glace at Montanvert was 2·25 feet.

† Travels in the Alps.

The following remarkable experiment of Mr. Hopkins of Cambridge*, which is considered by him to be confirmatory of the sliding theory of De Saussure as opposed to De Charpentier's dilatation theory, receives a ready explanation on the principles which I have laid down in this note. It is indeed a necessary result of them. "Mr. Hopkins placed a mass of rough ice, confined by a square frame or bottomless box, upon a roughly chiselled flag stone, which he then inclined at a small angle, and found that a slow but uniform motion was produced, when even it was placed at an inconsiderable slope." This motion, which Mr. Hopkins attributed to the dissolution of the ice in contact with the stone, would, I apprehend, have taken place if the mass had been of lead instead of ice; and it would have been but about half as fast, because the linear expansion of lead is only about half that of ice.

II. "Reply of the President and Council of the Royal Society to an application from the Lords of the Committee of Privy Council for Trade, on the subject of Marine Meteorological Observations."

[This Letter was communicated to the Society in pursuance of a resolution of the Council. The Secretary explained that it had been drawn up by the Treasurer, Colonel Sabine, and submitted, before final adoption by the Council, to several Fellows of the Society specially conversant with the subjects to which it refers.]

Royal Society, Somerset House,
February 22, 1855.

Sir,—In the month of June last, the Lords of the Committee of the Privy Council for Trade caused a letter to be addressed to the President and Council of the Royal Society, acquainting them that their Lordships were about to submit to Parliament an estimate for an Office for the Discussion of the Observations on Meteorology, to

* I have quoted the above account of it from Professor Forbes's book, p. 419.

be made at sea in all parts of the globe, in conformity with the recommendation of a conference held at Brussels in 1853; and that they were about to construct a set of forms for the use of that Office, in which they proposed to publish from time to time and to circulate such statistical results obtained by means of the observations referred to, as might be considered most desirable by men learned in the science of Meteorology, in addition to such other information as might be required for the purposes of Navigation.

Before doing so, however, their Lordships were desirous of having the opinion of the Royal Society, as to what were the great desiderata in Meteorological science; and as to the forms which may be best calculated to exhibit the great atmospheric laws which it may be most desirable to develope.

Their Lordships further state, that as it may possibly happen that observations on land upon an extended scale may hereafter be made and discussed in the same Office, it is desirable that the reply of the Royal Society should keep in view, and provide for such a contingency.

Deeply impressed with a sense of the magnitude and importance of the work which has been thus undertaken by Her Majesty's Government and confided to the Board of Trade, and fully appreciating the honour of being consulted, and the responsibility of the reply which they are called upon to make;—considering also that by including the contingency of *land* observations, the inquiry is, in fact, co-extensive with the requirements of Meteorology over all accessible parts of the earth's surface,—the President and Council of the Royal Society deemed it advisable, before making their reply, to obtain the opinion of those amongst their foreign members who are known as distinguished cultivators of Meteorological science, as well as of others in foreign countries, who either hold offices connected with the advancement of Meteorology, or have otherwise devoted themselves to this branch of science.

A circular was accordingly addressed to several gentlemen whose names were transmitted to the Board of Trade in June last, containing a copy of the communication from the Board of Trade, and a request to be favoured with any suggestions which might aid Her Majesty's Government in an undertaking which was obviously one of general concernment.

Replies in some degree of detail have been received from five of these gentlemen *, copies of which are herewith transmitted.

The President and Council are glad to avail themselves of this opportunity of expressing their acknowledgements to these gentlemen, and more particularly to Professor Dove, Director of the Meteorological Establishments and Institutions in Prussia, whose zeal for the advancement of Meteorology induced him to repair personally to England, and to join himself to the Committee by whom the present reply has been prepared. Those who are most familiar with the labours and writings of this eminent meteorologist will best be able to appreciate the value of his co-operation.

The President and Council have considered it as the most convenient course to divide their reply under the different heads into which the subject naturally branches. But before they proceed to treat of these, they wish to remark generally, that one of the chief impediments to the advancement of Meteorology consists in the very slow progress which is made in the transmission from one country to another of the observations and discussions on which, under the fostering aid of different Governments, so much labour is bestowed in Europe and America; and they would therefore recommend that such steps as may appear desirable should be taken by Her Majesty's Government, to promote and facilitate the mutual interchange of Meteorological publications emanating from the Governments of different countries.

Barometer.

It is known that considerable differences, apparently of a permanent character, are found to exist in the mean barometric pressure in different places; and that the periodical variations in the pressure in different months and seasons at the same place, are very different in different parts of the globe, both as respects period and amount; insomuch that in extreme cases, the variations have even opposite features in regard to period, in places situated in the same hemisphere and at equal distances from the equator.

For the purpose of extending our knowledge of the facts of these departures from the state of equilibrium, and of more fully investi-

* Dr. Erman of Berlin; Dr. Heis of Münster; Prof. Kreil of Vienna; Lieut. Maury of Washington; and M. Quetelet of Brussels.

gating the causes thereof, it is desirable to obtain, by means of barometric observations strictly comparable with each other, and extending over all parts of the globe accessible by land or sea, *tables*, showing the mean barometric pressure *in the year*, *in each month of the year*, and *in the four meteorological seasons*,—on land, at all stations of observation,—and at sea, corresponding to the middle points of spaces bounded by geographical latitudes and longitudes, not far distant from each other.

The manner of forming such tables from the marine observations which are now proposed to be made, by collecting together observations of the same month in separate ledgers, each of which should correspond to a *geographical space* comprised between specified meridians and parallels, and to a *particular month*, is too obvious to require to be further dwelt upon. The distances apart of the meridians and parallels will require to be varied in different parts of the globe, so that the magnitudes of the spaces which they enclose, and for each of which a table will be formed, may be more circumscribed, when the rapidity of the variation of the particular phenomenon to be elucidated is greatest in regard to geographical space. Their magnitude will also necessarily vary with the number of observations which it may be possible to collect in each space, inasmuch as it is well known that there are extensive portions of the ocean which are scarcely ever traversed by ships, whilst other portions may be viewed as the highways of a constant traffic.

The strict comparability of observations made in different ships may perhaps be best assured, by limiting the examination of the instruments to comparisons which it is proposed to make at the Kew Observatory, before and after their employment in particular ships. From the nature of their construction, the barometers with which Her Majesty's navy and the mercantile marine are to be supplied are not very liable to derangement, except from such accidents as would destroy them altogether. Under present arrangements they will all be carefully compared at Kew before they are sent to the Admiralty or to the Board of Trade; and similar arrangements may easily be made by which they may be returned to Kew for re-examination at the expiration of each tour of service. The comparison of barometers when embarked and in use, with standards, or supposed

standards, at ports which the vessels may visit, entails many inconveniences, and is in many respects a far less satisfactory method. The limitation here recommended is not, however, to be understood as applicable in the case of other establishments than Kew, where a special provision may be made for an equally careful and correct examination.

At land stations, in addition to proper measures to assure the correctness of the barometer and consequent comparability of the observations, care should be taken to ascertain by the best possible means (independently of the barometer itself), the height of the station above the level of the sea at some stated locality. For this purpose the extension of levels for the construction of railroads will often afford facilities.

It may be desirable to indicate some of the localities where the data, which tables such as those which have been spoken of would exhibit, are required for the solution of problems of immediate interest.

1°. It is known, that, over the Atlantic Ocean, a low mean annual pressure exists near the equator, and a high pressure at the north and south borders of the torrid zone (23° to 30° north and south latitudes); and it is probable that from similar causes similar phenomena exist over the corresponding latitudes in the Pacific Ocean: the few observations which we possess are in accord with this supposition; but the extent of space covered by the Pacific is large and the observations are few; they may be expected to be greatly increased by the means now contemplated. But it is particularly over the Indian Ocean, both at the equator and at the borders of the torrid zone, that the phenomena of the barometric pressure, not only annual but also monthly, require elucidation by observations. The Trade-winds, which would prevail generally round the globe if it were wholly covered by a surface of water, are interrupted by the large continental spaces in Asia and Australia, and give place to the phenomena of monsoons, which are the indirect results of the heating action of the sun's rays on those continental spaces. These are the causes of that displacement of the trade-winds, and substitution of a current flowing in another direction, which occasion the atmospheric phenomena over the Indian Ocean, and on the north and south sides of that Ocean, to be

different from those in corresponding localities over, and on either side of the equator in the Atlantic Ocean, and (probably generally also) in the Pacific Ocean.

It is important alike to navigation and to general science to know the limits where the phenomena of the trade-winds give place to those of the monsoons; and whether any and what variations take place in those limits in different parts of the year. *The barometric variations are intimately connected with the causes of these variations, and require to be known for their more perfect elucidation.*

The importance, indeed, of a full and complete knowledge of the variations which take place in the limits of the trade-winds generally in both hemispheres, at different seasons of the year, has long been recognized. On this account, although the present section is headed " Barometer," it may be well to remark here, that it is desirable that the forms supplied to ships should contain headings, calling forth a special record of the latitude and longitude where the trade wind is first met with, and where it is first found to fail.

2°. The great extent of continental space in Northern Asia causes, by reason of the great heat of the summer and the ascending current produced thereby, a remarkable diminution of atmospheric pressure in the summer months, extending in the north to the Polar Sea, and on the European side as far as Moscow. Towards the east it is known to include the coasts of China and Japan, but the extent of this great diminution of summer pressure beyond the coasts thus named is not known. A determination of the monthly variation of the pressure over the adjacent parts of the Pacific Ocean is therefore a desideratum; and for the same object it is desirable to have a more accurate knowledge than we now possess of the prevailing direction of the wind in different seasons in the vicinity of the coasts of China and Japan.

3°. With reference to regions or districts of increased or diminished *mean annual* pressure, it is known that in certain districts in the temperate and polar zones, such as in the vicinity of Cape Horn extending into the antarctic polar Ocean, and in the vicinity of Iceland, the mean annual barometric pressure is *considerably* less than the average pressure on the surface of the globe generally; and that anomalous differences, also of considerable amount, exist in the

mean annual pressure in different parts of the arctic ocean. These all require special attention, with a view to obtain a more perfect knowledge of the facts, in regard to their amount, geographical extension, and variation with the change of seasons, as well as to the elucidation of their causes.

Dry Air and Aqueous Vapour.

The apparently anomalous variations which have been noticed to exist in the mean annual barometric pressure, and in its distribution in the different seasons and months of the year, are also found to exist in each of the two constituent pressures which conjointly constitute the barometric pressure. In order to study the problems connected with these departures from a state of equilibrium under their most simple forms,—and generally for the true understanding of almost all the great laws of atmospheric change,—it is necessary to have a separate knowledge of the two constituents (viz. the pressures of the dry air and of the aqueous vapour) which we are accustomed to measure together by the barometer. This separate knowledge is obtained by means of the hygrometer, which determines the elasticity of the vapour, and leads to the determination of that of the dry air, by enabling us to deduct the elasticity of the vapour from that of the whole barometric pressure. It is therefore extremely desirable that tables, similar to those recommended under the preceding head of the barometer, should be formed at every land station, and over the ocean at the centres of geographical spaces bounded by certain values of latitude and longitude, for the ***annual***, ***monthly***, and ***season*** pressures,—1. Of the aqueous vapour; and 2. Of the dry air; each considered separately. Each of the said geographical spaces will require its appropriate ledger for each of the twelve months.

It may be desirable to notice one or two of the problems connected with extensive and important atmospherical laws which may be materially assisted by such tables.

1°. By the operation of causes which are too well-known to require explanation here, the dry air should always have a minimum pressure in the hottest months of the year. But we know that there are places where the contrary prevails, namely, that the pressure of the dry air is greater in summer than in winter. We also

know that when comparison is made between places in the same latitude, and having the same, or very nearly the same, differences of temperature in summer and in winter, the differences between the summer and winter pressures of the dry air are found to be subject to many remarkable anomalies. The variations in the pressure of the dry air do not therefore, as might be at first imagined, depend altogether on the differences between the summer and winter temperatures at the places where the variations themselves occur. The increased pressure in the hottest months appears rather to point to the existence of an overflow of air in the higher regions of the atmosphere from *lateral sources*; the statical pressure at the base of the column being increased by the augmentation of the superincumbent mass of air arising from an influx in the upper portion. Such lateral sources may well be supposed to be due to *excessive ascensional currents* caused by *excessive summer heats* in certain places of the globe (as, for example, in Central Asia). Now the lateral overflow from such sources, traversing in the shape of currents the higher regions of the atmosphere, and encountering the well-known general current flowing from the equator towards the pole, has been recently assigned with considerable probability (derived from its correspondence with many otherwise anomalous phenomena already known, and which all receive an explanation from such supposition) to be the original source or primary cause of the *rotating storms* or *cyclones*, so well known in the West Indies and in China under the names of hurricanes and typhoons. A single illustration may be desirable. Let it be supposed that such an excessive ascensional current exists over the greatly heated parts of Asia and Africa in the northern tropical zone,—giving rise, in the continuation of the same zone over the Atlantic Ocean, to a lateral current in the upper regions; this would then be a current prevailing in those regions from east to west: and it would encounter over the Atlantic Ocean the well-known upper current proceeding from the equator towards the pole, which is a current from the south-west. An easterly current impinging on a south-west current may give rise, by well-known laws, to a rotatory motion in the atmosphere, of which the direction may be the same as that which characterises the cyclones of the northern hemisphere. To test the accuracy of this explanation, we desire to be acquainted with the

variations which the ***mean pressure of the dry air undergoes in the different seasons*** in the part of the globe, where, according to this explanation, considerable variations having particular characters ought to be found.

2°. We have named one of the explanations which have been recently offered of the primary cause of the northern cyclones. Another mode of explanation has been proposed, by assuming the condensation of large quantities of vapour, and the consequent influx of air to supply the place. In such case the phenomena are to be tested in considerable measure by the variations which the *other constituent* of the barometric pressure, namely, the *aqueous vapour*, undergoes.

3°. The surface of sea in the southern hemisphere *much* exceeds that in the northern hemisphere. It is therefore probable that at the season when the sun is over the southern hemisphere, evaporation over the whole surface of the globe is more considerable than in the opposite season when the sun is over the northern hemisphere. Supposing the pressure of the dry air to be a constant, the difference of evaporation in the two seasons may thus produce for the whole globe an *annual barometric variation*, the aggregate barometric pressure over the *whole* surface being highest during the northern winter. The separation of the barometric pressure into its two constituent pressures would give direct and conclusive evidence of the cause to which such a barometric variation should be ascribed. It would also follow, that evaporation being greatest in the south, and condensation greatest in the north, the water which proceeds from south to north in a state of vapour, would have to return to the south in a liquid state, and might possibly exert some discernible influence on the currents of the ocean. The tests by which the truth of the suppositions thus advanced may be determined, are the variations of the meteorological elements in different seasons and months, determined by methods and instruments strictly comparable with each other, and arranged in such tables as have been suggested. A still more direct test would indeed be furnished by the fact (if it could be ascertained), that the quantity of rain which falls in the northern is greater than that which falls in the southern hemisphere; and by examining its distribution into the different months and seasons

of its occurrence. Data for such conclusions are as yet very insufficient; they should always, however, form a part of the record at all land stations where registers are kept.

In order that all observations of the elasticity of the aqueous vapour may be strictly comparable, it is desirable that all should be computed by the same tables; those founded upon the experiments of MM. Regnault and Magnus may be most suitably recommended for this purpose, not only on their general merits, but also as being likely to be most generally adopted by observers in other countries.

Temperature of the Air.

Tables of the mean temperature of the air in the year, and in the different months and seasons of the year, at above 1000 stations on the globe, have recently been computed by Professor Dove, and published under the auspices of the Royal Academy of Sciences at Berlin. This work,—which is a true model of the method in which a great body of meteorological facts, collected by different observers and at different times, should be brought together and coordinated,—has conducted, as is well known, to conclusions of very considerable importance in their bearing on climatology, and on the general laws of the distribution of heat on the surface of the globe. These tables have, however, been formed exclusively from observations made *on land.* For the completion of this great work of physical geography, there is yet wanting a similar investigation for the *oceanic* portion; and this we may hopefully anticipate as likely to be now accomplished by means of the marine observations about to be undertaken. In the case of the temperature of the air, as in that of the atmospheric pressure previously adverted to, the centres of geographical spaces bounded by certain latitudes and longitudes will form points of concentration for observations, which may be made within those spaces, not only by the same but also by different ships; provided that the system be steadily maintained of employing only instruments which shall have been examined, and their intercomparability ascertained, by a competent and responsible Authority;—and provided that no observations be used but those in which careful attention shall have been given to the precautions which it will be necessary to adopt, for the purpose of obtaining the correct knowledge of the temperature of the external air, amidst the

many disturbing influences from heat and moisture so difficult to escape on board ship. In this respect additional precautions must be used if *night observations* are to be required, since the ordinary difficulties are necessarily much enhanced by the employment of artificial light. Amongst the instructions which will be required, perhaps there will be none which will need to be more carefully drawn, than those for obtaining the correct temperature of the external air under the continually varying circumstances that present themselves on board ship.

In regard to *land stations*, Professor Dove's tables have shown that data are still pressingly required from the British North American possessions intermediate between the stations of theArctic Expeditions and those of the United States; and that the deficiency extends across the whole North American Continent in those latitudes from the Atlantic to the Pacific. Professor Dove has also indicated as desiderata, observations at the British Military stations in the Mediterranean (Gibraltar, Malta and Corfu), and around the Coasts of Australia and New Zealand: also that *hourly* observations, continued for at least one year, are particularly required at some one station in the West Indies, to supply the diurnal corrections for existing observations.

Whilst the study of the distribution of heat at the surface of the globe has thus been making progress, in respect to the *mean annual temperature* in different places, and to its *periodical variations* in different parts of the year at the same place, the attention of physical geographers has recently been directed (and with great promise of important results to the material interests of men as well as to general science), to the causes of those fluctuations in the temperature, or departures from its mean or normal state at the same place and at the same period of the year, which have received the name of "non-periodic variations." It is known that these frequently affect extensive portions of the globe at the same time; and are generally, if not always, accompanied by a fluctuation of an opposite character, prevailing at the same time in some adjoining but distant region; so that by the comparison of synchronous observations a progression is traceable, from a locality of maximum increased heat in one region, to one of maximum diminished heat in another region. For the elucidation of the non-periodic variations even

monthly means are insufficient; and the necessity has been felt of computing the mean temperatures for periods of much shorter duration. The Meteorological Institutions of those of the European States which have taken the foremost part in the prosecution of meteorology, have in consequence adopted *five-day means*, as the most suitable intermediate gradation between daily and monthly means: and as an evidence of the conviction which is entertained of the value of the conclusions to which this investigation is likely to lead, it has been considered worth while to undertake the prodigious labour of calculating the five-day means of the most reliable existing observations during a century past. This work is already far advanced; and it cannot be too strongly recommended, that at all fixed stations, where observations shall hereafter be made with sufficient care to be worth recording, five-day means may invariably be added to the daily, monthly, and annual means into which the observations are usually collected. The five-day means should always commence with January 1, for the purpose of preserving the uniformity at different stations, which is essential for comparison: in leap years, the period which includes the 29th of February will be of six days.

In treating climatology as a *science*, it is desirable that some correct and convenient mode should be adopted, for computing and expressing the *comparative variability* to which the temperature in different parts of the globe, and in different parts of the year in the same place, is subject from non-periodic causes. The *probable variability*, computed on the same principle as the *probable error* of each of a number of independent observations, has recently been suggested as furnishing an index "of the probable daily non-periodic variation" at the different seasons of the year; and its use in this respect has been exemplified by calculations of the "index" from the five-day means of twelve years of observations at Toronto, in Canada (Phil. Trans. 1853, Art. V.). An index of this description is of course of absolute and general application; supplying the means of comparing the probable variability of the temperature in different seasons at *different places* (where the same method of computation is adopted) as well as at the *same place*. It is desirable that this (or some preferable method if such can be devised for obtaining the same object) should be adopted by those who may desire

to make their observations practically useful for sanitary or agricultural purposes, or for any of the great variety of objects for which climatic peculiarities are required to be known. Having these three data, viz. the mean annual temperature,—its periodical changes in respect to days, months, and seasons,—and the measure of its liability to non-periodic (or what would commonly be called, irregular) variations,—we may consider that we possess as complete a representation of the climate of any particular place (so far as temperature is concerned), as the present state of our knowledge permits.

It is obvious that much of what has been said under this article is more applicable to land than to sea observations; but the letter of the Board of Trade, to which this is a reply, requests that both should be contemplated.

Temperature of the Sea, and Investigations regarding Currents.

It is unnecessary to dwell on the practical importance to *navigation* of a correct knowledge of the currents of the ocean; their direction, extent, velocity, and the temperature of the surface water relatively to the ordinary ocean temperature in the same latitude; together with the variations in all these respects which currents experience in different parts of the year, and in different parts of their course. As the information on these points, which may be expected to follow from the measures adopted by the Board of Trade, must necessarily depend in great degree on the *intelligence*, as well as the *interest* taken in them by the observers, it is desirable that the instructions to be supplied with the meteorological instruments should contain a brief summary of what is already known in regard to the principal oceanic currents; accompanied by charts on which their supposed limits in different seasons, and the variations in those limits which may have been observed in particular years, may be indicated, with notices of the particularities of the temperature of the surface-water by which the presence of the current may be recognised. Forms will also be required for use in such localities, in which the surface temperatures may be recorded at hourly or half-hourly intervals, with the corresponding geographical positions of the ship, as they may be best inferred from observation and reckoning. For such localities also it will be necessary that the tables, into which the

observations of different ships at different seasons are collected, should have their bounding lines of latitude and longitude brought nearer together than may be required for the ocean at large.

In looking forward to the results which are likely to be obtained by the contemplated marine observations, it is reasonable that those which may bear practically on the interests of navigation should occupy the first place; but, on the other hand, it would not be easy to over-estimate the advantages to physical geography, of general tables of the surface temperature of the ocean in the different months of the year, exhibiting, as they would do, its normal and its abnormal states, the mean temperature of the different parallels, and the deviations therefrom, whether permanent, periodical, or occasional. The knowledge which such tables would convey is essentially required for the study of climatology *as a science*.

The degree in which climatic variations extending over large portions of the earth's surface may be influenced by the variable phenomena of oceanic currents in different years, may perhaps be illustrated by circumstances of known occurrence in the vicinity of our own coasts. The admirable researches of Major Rennell have shown that in ordinary years, the warm water of the great current known by the name of the Gulf-stream is not found to the east of the meridian of the Azores; the sea being of ordinary ocean temperature for its latitude at all seasons and in every direction, in the great space comprised between the Azores, and the coasts of Europe and North Africa: but Major Rennell has also shown that on two occasions, viz. in 1776 and in 1821–1822, the warm water by which the Gulf-stream is characterised throughout its whole course (*being several degrees* above the ordinary ocean temperature in the same latitude), was found to extend across this great expanse of ocean, and in 1776 (in particular) was traced (by Dr. Franklin) quite home to the coast of Europe. The presence of a body of unusually heated water, extending for several hundred miles both in latitude and in longitude, and continuing for several weeks, at a season of the year when the prevailing winds blow from that quarter on the coasts of England and France, can scarcely be imagined to be without a considerable influence on the relations of temperature and moisture in those countries. In accordance with this supposition,

we find in the Meteorological Journals of the more recent period (which are more easily accessible), that the state of the weather in November and December 1821 and January 1822 was so unusual in the southern parts of Great Britain and in France, as to have excited general observation; we find it characterised as "most extraordinarily hot, damp, stormy, and oppressive," that "the gales from the W. and S.W. were almost without intermission," "the fall of rain was excessive" and "the barometer lower than it had ever been known for 35 years before."

There can be little doubt that Major Rennell was right in ascribing the unusual extension of the Gulf-stream in particular years to its greater initial velocity, occasioned by a more than ordinary difference in the levels of the Gulf of Mexico and of the Atlantic in the preceding summer. An unusual height of the Gulf of Mexico at the head of the stream, or an unusual velocity of the stream at its outset in the Strait of Florida, are facts which may admit of being recognised by properly directed attention; and as these must precede, by many weeks, the arrival of the warm water of the stream at above 3000 miles distance from its outset, and the climatic effects thence resulting, it might be possible to anticipate the occurrence of such unusual seasons upon our coasts.

Much, indeed, may undoubtedly be done towards the increase of our partial acquaintance with the phænomena of the Gulf-stream, and of its counter currents, by the collection and coordination of observations made by casual passages of ships in different years and different seasons across different parts of its course; but for that full and complete knowledge of all its particulars, which should meet the maritime and scientific requirements of the period in which we live, we must await the disposition of Government to accede to the recommendation, so frequently made to them by the most eminent hydrographical authorities, of a specific survey of the stream by vessels employed for that special service. What has been recently accomplished by the Government of the United States in this respect, shows both the importance of the inquiry and the great extent of the research; and lends great weight to the proposition, which has been made to Her Majesty's Government on the part of the United States, for a joint survey of the whole stream by vessels of the two countries. The establishment of an office under the Board of Trade

specially charged with the reduction and coordination of such data may materially facilitate such an undertaking.

Storms or Gales.

It is much to be desired both for the purposes of navigation and for those of general science, that the captains of Her Majesty's ships and masters of merchant vessels should be correctly and thoroughly instructed in the methods of distinguishing *in all cases* between the rotatory storms or gales, which are properly called *Cyclones,* and gales of a more ordinary character, but which are frequently accompanied by a veering of the wind, which under certain circumstances might easily be confounded with the phenomena of Cyclones, though due to a very different cause. It is recommended therefore that the instructions, proposed to be given to ships supplied with meteorological instruments, should contain clear and simple directions for distinguishing *in all cases* and *under all circumstances* between these two kinds of storms; and that the forms to be issued for recording the meteorological phenomena during great atmospheric disturbances should comprehend a notice of all the particulars which are required for forming a correct judgment in this respect.

Thunder-storms.

It is known that in the high latitudes of the northern and southern hemispheres thunder-storms are almost wholly unknown; and it is believed that they are of very rare occurrence over the ocean in the middle latitudes when distant from continents. By a suitable classification and arrangement of the documents which will be henceforward received by the Board of Trade, statistical tables may in process of time be formed, showing the comparative frequency of these phenomena in different parts of the ocean, and in different months of the year.

It is known that there are localities on the globe where, during certain months of the year, thunder-storms may be considered as a periodical phenomenon of daily occurrence. In the Port Royal Mountains in Jamaica, for example, thunder-storms are said to take place *daily* about the hour of noon from the middle of November to the middle of April. It is much to be desired that a full and

precise account of such thunder-storms, and of the circumstances in which they appear to originate, should be obtained.

In recording the phenomena of thunder and lightning it is desirable to state the duration of the interval between the flashes of lightning and the thunder which follows. This may be done by means of a seconds-hand watch, by which the time of the apparition of the flash, and of the commencement (and of the conclusion also) of the thunder may be noted. The interval between the flash, and the commencement of the thunder, has been known to vary in different cases, from less than a single second to between 40 and 50 seconds, and even on very rare occasions to exceed 50 seconds. The two forms of ordinary lightning, viz. zigzag (or forked) lightning, and sheet lightning, should always be distinguished apart; and particular attention should be given both to the observation and to the record, in the rare cases when zigzag lightning either bifurcates, or returns upwards. A special notice should not fail to be made when thunder and lightning, or either separately, occur in a perfectly cloudless sky. When globular lightning (balls of fire) are seen, a particular record should be made of all the attendant circumstances. These phenomena are known to be of the nature of lightning, from the injury they have occasioned in ships and buildings that have been struck by them; but they differ from ordinary lightning not only by their globular shape, but by the length of time they continue visible, and by their slow motion. They are said to occur sometimes without the usual accompaniments of a storm, and even with a perfectly serene sky. Conductors are now so universally employed in ships, that it may seem almost superfluous to remark that, should a ship be struck by lightning, the most circumstantial account will be desirable of the course which the lightning took, and of the injuries it occasioned; or to remind the seaman that it is always prudent, after such an accident has befallen a ship, to distrust her compasses until it has been ascertained that their direction has not been altered. Accidents occurring *on land* from lightning will, of course, receive the fullest attention from meteorologists who may be within convenient distance of the spot.

Auroras and Falling Stars.

Auroras are of such rare occurrence in seas frequented by ships

engaged in commerce, that it may seem superfluous to give any particular directions for their observation *at sea*; and land observatories are already abundantly furnished with such. It is, of course, desirable that the meteorological reports received from ships should always contain a notice of the time and place where Auroras may be seen, and of any remarkable features that may attract attention.

The letter from Professor Heis, which is one of the foreign communications annexed, indicates the principal points to be attended to in the instructions which it may be desirable to draw up for the observation of "Falling Stars." For directions concerning Halos and Parhelia, a paper by Monsieur Bravais in the 'Annuaire Météorologique de la France' for 1851, contains suggestions which will be found of much value.

Charts of the Magnetic Variation.

Although the variation of the compass does not belong in strictness to the domain of meteorology, it has been included, with great propriety, amongst the subjects treated of by the Brussels Conference, and should not therefore be omitted here. It is scarcely necessary to remark, that whatever may have been the practice in times past, when the phenomena of the earth's magnetism were less understood than at present, it should in future be regarded as indispensable, that variation-charts should always be constructed for a *particular epoch*, and that *all parts* of the chart should show *the variation corresponding to the epoch for which it is constructed.* Such charts should also have, either engraved on the face or attached in some convenient manner, a table, showing the approximate annual rate of the secular change of the variation in the different latitudes and longitudes comprised: so that, by means of this table, the variation taken from the chart for any particular latitude and longitude may be corrected to the year for which it is required, if that should happen to be different from the epoch for which the chart is constructed.

A valuable service would be rendered to this very important branch of hydrography, if, under the authority of the new department of the Board of Trade, variation-charts for the North and South Atlantic Oceans, for the North and South Pacific Oceans, for the Indian Ocean, and for any other localities in which the require-

ments of navigation might call for them, were published at *stated intervals*, corrected for the secular change that had taken place since the preceding publication. Materials would be furnished for this purpose by the observations which are now intended to be made, supposing them to be collected and suitably arranged with proper references to date and to geographical position, and to the original reports in which the results and the data on which they were founded were communicated. By means of these observations the tables of approximate correction for secular change might also be altered from time to time as occasion should require, since the rate of secular change itself is not constant.

All observed variations, communicated or employed as data upon which variation-charts may be either constructed or corrected, should be accompanied by other observational data (the nature of which ought now to be well understood), for correcting the observed variation for the error of the compass occasioned by the ship's iron. It is also strongly recommended that no observations be received as data for the formation or correction of variation-charts, but such as are accompanied by a detailed statement of the principal elements both of observation and of calculation. Proper forms should be supplied for this purpose; or, what is still better, books of blank forms may be supplied, in which the observations themselves may be entered, and the calculation performed by which the results are obtained. Such books of blank forms would be found extremely useful both for the variation of the needle, and for the chronometrical longitude; (as well as for lunar observations, if the practice of lunar observations be not, as there is too much reason to fear it is, almost wholly discontinued). By preparing and issuing books of blank forms suitable for these purposes, and by requesting their return in accompaniment with the other reports to be transmitted to the Board of Trade at the conclusion of a voyage, the groundwork would be laid for the attainment of greatly improved habits of accuracy in practical navigation in the British mercantile marine.

The President and Council are aware that they have not exhausted the subject of this reply in what they have thus directed me to address to you; but they think that perhaps they have noticed as many points as may be desirable for *present* attention; and they desire me to add, that they will be at all times ready to resume the

consideration if required, and to supply any further suggestions which may appear likely to be useful.

I have the honour to be, Sir,
Your obedient Servant,
W. SHARPEY,
Sec. R.S.

To the Secretary of the Lords of the Committee of Privy Council for Trade.

April 26, 1855.

Sir BENJAMIN BRODIE, Bart., V.P., in the Chair.

Dr. Dickenson was admitted into the Society.

The following communications were read:—

I. A letter from EDWIN CANTON, Esq., accompanying the collection of autograph letters to which it refers. Communicated by Professor STOKES, Sec. R.S.

"4 Montagu Street, Russell Square,
March 29, 1855.

"DEAR SIR,—Will you do me the favour of presenting to the Royal Society, for their acceptance, the accompanying collection of autograph letters from Franklin, Priestley, Sir J. Banks and others, together with several documents which were formerly in the possession of my great-grandfather, John Canton, F.R.S.

"The collection was given to me when a lad of about fourteen years of age by my uncle (Mr. Nathaniel Canton), who resides in the neighbourhood of Spital Square. It was received by him from his father (Mr. William Canton), whose residence was the same as that now occupied by his son.

"I mention, particularly, the time when the letters came into my possession in explanation of the circumstance of some of them being wanting in address or superscription, for, being but a lad at the

time of their receipt, I was unaware of the value which attaches to a 'direction,' 'post-mark,' &c., and, in my ignorance, I tore away, in a few instances, these important aids to proof of authenticity.

"Accompanying this note and the above collection is one of the balls of 'elastic substance,' mentioned by Sir Joseph Banks in a communication to my great-grandfather. Should the Royal Society deem this, too, worthy of being in their possession, I shall have much pleasure in their accepting it.

"I am, dear Sir,
"Yours very truly,
"EDWIN CANTON."

"*To C. R. Weld, Esq.*,
Royal Society."

"P.S. In thanking you, Sir, for your suggestion that I should *bequeath* the letters, &c. to the Royal Society, I beg to say that I shall feel myself much gratified in finding the present to be one which they will *now* do me the honour of accepting; and in conclusion, may I beg that you will oblige me by retaining, yourself, the seal, in which you take an interest?"

II. "Some Observations on the Ova of the Salmon, in relation to the distribution of Species; in a letter addressed to CHARLES DARWIN, Esq., M.A., V.P.R.S. &c." By JOHN DAVY, M.D., F.R.SS. Lond. & Edinb., Inspector-General of Army Hospitals. Received March 27, 1855.

In this paper the author describes a series of experiments on the ova of the Salmon, made with the intent of ascertaining their power of endurance under a variety of circumstances without loss of life, with the expectation suggested by Mr. Darwin, that the results might possibly throw some light on the geographical distribution of fishes.

The details of the experiments are given in five sections. The results obtained were the following:—

1. That the ova of the Salmon in their advanced stage can be exposed only for a short time to the air if dry, at ordinary temperatures, without loss of life; but for a considerable time, if the temperature be low, and if the air be moist; the limit in the former case

not having exceeded an hour, whilst in the latter it has exceeded many hours.

2. That the vitality of the ova was as well preserved in air saturated with moisture, as it would have been had they been in water.

3. That the ova may be included in ice without loss of vitality, provided the temperature is not so low as to freeze them.

4. That the ova, and also the fry recently produced, can bear for some time a temperature of about 80° or 82° in water, without materially suffering; but not without loss of life, if raised above 84° or 85°.

5. That the ova and young fry are speedily killed by a solution of common salt nearly of the specific gravity of sea-water, viz. 1026; and also by a weaker solution of specific gravity 1016.

Finally, in reference to the inquiry regarding the distribution of the species of fishes, he expresses his belief that some of the results may be of useful application, especially those given in the second and third sections; inferring, that as in moist air, the vitality of the ova is capable of being long sustained, they may during rain or fog be conveyed from one river or lake to another adhering to some part of an animal, such as a Heron or Otter, and also during a time of snow or frost; and, further, that other of the results may be useful towards determining the fittest age of ova for transport for the purpose of stocking rivers, and likewise as a help to explain the habitats, and some of the habits of the migratory species.

III. "Observations on the Anatomy and Affinities of the *Phyllirrhoë bucephala* (Peron)." By JOHN DENIS MACDONALD, Esq., R.N., Assistant-Surgeon of H.M.S.V. 'Torch.' Communicated by Sir W. BURNETT, K.C.B. Received March 30, 1855.

As the true position of Peron's genus *Phyllirrhoë*, and even the very existence of the animals composing it, have been matters of doubt to zoologists, during a late cruise to the Fiji Islands I determined to ply the towing-net with a little more diligence than usual, hoping to obtain a few of these almost hypothetical beings, and was rewarded by the capture of many specimens.

Some were taken in the neighbourhood of Lord Howe's Island, S. lat. 31° 31″, E. long. 159° 5″, some near Norfolk Island, S. lat. 29° 2″, E. long. 168° 2″, and others, although in smaller numbers, in different parts of our track. They generally made their appearance after dusk in the evening, and presented a great diversity in size, form and other external characters, which is due to changes in the muscular system, a variable amount of pigment spots, &c. Indeed at first I fully believed that several distinct species had been brought up together, but this idea was abandoned when I observed the most dissimilar forms gradually assume so close a resemblance to each other, as ultimately to render it difficult to distinguish them.

From these facts I am much inclined to think that the three species described by Quoy and Gaimard, viz. *P. amboinensis*. *P. punctulata* and *P. rubra, P. Lichtensteinii* (*Eurydice Lichtensteinii* of Eschscholtz) and *P. rosea* of D'Orbigny, are all referable to Peron's original species *P. bucephala*.

The body of *Phyllirrhoë* is elongated in form and compressed laterally, presenting for description an anterior and posterior extremity, a right and left surface, and a dorsal and ventral border. The head is surmounted by two lengthy, somewhat flattened and acuminate tentacula; the eyes lie beneath the skin, not being visible externally, and the mouth is in the form of a short truncated proboscis, with a vertical opening. The oval-shaped body is on an average about one inch and a half in length, which is something over twice the measurement from the dorsal to the ventral border taken at the middle or broadest part. The tail is quadrilateral in figure, gradually widening towards its posterior border, which is exceedingly thin. The outer integument is perfectly transparent and lined by muscular bundles, disposed longitudinally, and somewhat more than their own breadth apart. These communicate with one another by oblique branching slips, which thus form a kind of network enclosing long lozenge-shaped spaces. Here and there nerve-trunks of considerable size accompany the longitudinal bundles, dividing off into smaller twigs, which distribute themselves at pretty equal distances in a direction more or less perpendicular to that of the muscular fibres. Scattered about at irregular intervals amongst these structures are numerous reddish-brown pigment-spots, in the centre of each of which a clear vesicle is generally distinguishable. As above

alluded to, the actual tint of this pigment, and the relative number of spots deposited within a certain space, determine both the general quality and the depth of colour which are found to vary so much in different specimens of *Phyllirrhoë.*

The alimentary canal of this creature consists of a muscular tube lined with mucous membrane, extending without flexure from the mouth to the vent. It commences anteriorly in an oral dilatation, in connexion with which we notice a pair of lateral horny jaws articulated with each other superiorly, and beset with very minute and sharp-pointed teeth along the cutting edge, altogether much resembling those of *Glaucus*, and a lingual ribbon gradually increasing in diameter from before backwards, and supporting a pavement of long, conical, flattened and gracefully curved teeth with fine denticulations at the base. The central series of plates being symmetrical, the large tooth in each takes up a middle position, but in the lateral plates it inclines to the inner side. In some examples I have observed certain lobulated bodies lying in contact with the buccal mass, and which I am disposed to regard as salivary glands. The œsophagus is short, and suddenly expands into a moderately large stomach; and the latter, having received the biliary ducts near its posterior extremity, is continued into the rectum, which passes directly backwards some little distance, and ends in the anus, on the right side of the body, at the union of its posterior and middle thirds. The liver in *Phyllirrhoë* consists of four elongated, tubular, and sacculated portions or lobes, disposed along the borders of the body, two lying above and two below the alimentary canal. Each of the superior hepatic glands opens by a distinct duct into the supero-posterior part of the stomach, while the ducts of the inferior ones unite to form a common tube joining it at its infero-posterior part. The opposite or cæcal extremities of the two anterior hepatic lobes end in the neighbourhood of the head, while those of the others extend to within a short distance of the tail. The secreting cells of these organs are of a rounded or polyhedral form, containing, besides the nucleus, a reddish-brown pigment and fatty globules.

Phyllirrhoë possesses a simple systemic heart, consisting of a single auricle and ventricle. This organ lies upon the stomach, between the ducts of the two superior biliary glands; and a large vessel or sinus, with many circular constrictions in its walls, may be traced

towards the auricle, bringing back the aërated blood from the hinder extremity of the body. There are no visible respiratory organs, but it is probable that the cutaneous surface permits of the necessary exposure of the blood to the air contained in the surrounding medium.

The nervous system is well developed. The supra- and subœsophageal ganglia, with their commissural chords, form a close ring round the gullet immediately behind the buccal mass. The auditory sacs, which are filled with vibratory otokonia, appear to lie between both sets of ganglia, and the rudimentary visual organs, consisting each of a simple cell containing a refracting globule imbedded in black pigment, are also in contact with the nervous matter. Besides the actual distribution of the nerves given off from the cephalic ganglia, I noticed nodules of neurine lying at the base of the tentacula, communicating by commissural threads, and sending off each a principal nerve to the corresponding tentacle. The ganglion-globules were lined with a reddish-coloured pigment, deposited round the vesicular nuclei, and when twigs are given off from the smaller nerves, both the homogeneous neurilemma and the contained nervous matter break up like a dividing vessel, without preserving the individuality of distinct nerve tubes.

The sexes are combined in *Phyllirrhoë*, the male and female generative openings lying close together on the right side of the body in the inferior gastero-hepatic space, and before the anal aperture. The ovaries lie in the inferior recto-hepatic space, varying in number from two to five, in general. They are dark-coloured, subrotund, and finely lobulated bodies, from the fore part of each of which a very delicate duct arises, and all the ducts unite to form a single tube, with a trifling increase in its diameter. This common oviduct, lined by a pavement of transparent epithelial cells, passes forwards beneath the stomach in a flexuous manner; and in the inferior gastero-hepatic space, it first unites with the duct of the testis and again continues its devious course until it ends in the fundus of a much larger tube, whose lining membrane is armed with numerous conical and tooth-like processes, and to this is appended a long cæcal process much resembling the spermatheca of *Helix* for example. The external orifice of the male generative apparatus lies immediately posterior to that of the female organs. The testis is rather

small, subglobular in form, and closely connected with a short twisted tube*, much dilated at the middle part, and coated over with a layer of dark pigment cells. It is with this tube, as above noticed, the small oviduct communicates, in order, as it would seem, to permit of self-impregnation, or to answer some other purpose, with the nature of which we are unacquainted; but there is also an intromittent organ, which, however, I have never seen properly exserted.

As to the affinities of *Phyllirrhoë* with Gasteropods, it may be observed that the animal is bisexual, that the eyes, like those of *Glaucus* and *Ianthina*, are very small and rudimentary, being closely applied to the ganglia of the brain, after the manner of the acoustic sacs, and that both *Phyllirrhoë* and *Glaucus* agree in possessing two lateral horny jaws, articulated with each other superiorly, and bordered with minute conical teeth.

In the *Glaucidæ*, the branchiæ, which consist of simple papillary projections of the skin, are distributed in an equable manner over the dorsal region of the body; and any deviation from this arrangement would naturally tend, either to a more definite localization, or still further dispersion. It is the latter modification which appears to have taken place in *Phyllirrhoë*; so that its respiratory vessels ramify minutely through the common integument, just as the vascular trunks analogous to those which break up in the pectinate gill, adapted for aquatic breathing, are subdivided, and spread themselves over the smooth walls of the lung-chamber in Pulmonifera.

As respects its affinity to the Pteropods, here too the lateral jaws of *Phyllirrhoë* must be borne in mind, together with the almost complete suppression of the organs of vision. It is worthy of note also, that its acoustic capsules contain otokonia, as in Pteropoda, instead of single globular otolithes like those of *Glaucus*, and there is some reason to believe that the long tentacula, so called, are the homologues of the cephalic fins of Pteropods.

The particular features of *Phyllirrhoë*, expressed in the last paragraph, also serve to distinguish it from the Heteropoda, but it somewhat approximates this order in the general conformation of its body, which is elongated, laterally compressed, and presents a kind of proboscis at the anterior, and a rudder-fin at the posterior extremity. There is also, as it would appear to be, a small remnant of the foot

* I have distinctly traced the homologue of this tube in *Pteropoda*, *Heteropoda*, and the *Gasteropoda* proper.

on the inferior thin margin of the body, and the lateral undulatory motion of the animal in the water exactly resembles that of *Cerophora*, or *Carinaria*.

The heart of *Phyllirrhoë*, in common with that of Heteropods in general, holds a dorsal position. The auricle lies posterior to the ventricle, as in *Cerophora* and *Firola*, but the reverse is the case in *Atlanta* and *Carinaria*, the difference being due to the relation which the respiratory surface bears to the heart itself, lying in every case on the auricular side. Moreover it is remarkable that the rectum is directed backwards in the former instances, but turns forwards in the latter, taking an opposite course to that of the circulation through the heart.

It may be observed in conclusion, that in Heteropoda the viscera are closely packed together so as to occupy the smallest possible space, while they are widely distributed through the abdomen in *Phyllirrhoë*; thus, again, calling to mind its relationship to the Pteropoda.

This paper is illustrated with drawings representing the animal described and some of the details of its internal structure.

IV. "Brief sketch of the Anatomy of a new genus of pelagic Gasteropoda, named *Jasonilla*." By John Denis Macdonald, Esq., R.N. Communicated by Sir W. Burnett, K.C.B. Received March 30, 1855.

This communication refers to a remarkable genus of pelagic Gasteropoda, characterized, like *Macgillivraya* and *Cheletropis*, by the presence of ciliated cephalic appendages, but having, as in the present instance, a beautifully transparent, cartilaginous and perfectly symmetrical shell. The author has seen but one species, which was frequently taken between Port Jackson and the Isle of Pines.

The shell resembles that of *Argonauta* in shape, is less than one-eighth of an inch in diameter, and the little animal, when fully retracted, occupies but a small portion of its cavity. The margin of the mantle is of considerable thickness, containing loosely-packed cells, similar to those of the middle or operculigerous lobe of many Pteropods. About eight ciliated arms, identical in character with those of *Macgillivraya*, &c., encircle the head, including the mouth, which

is furnished with two massive lateral jaws bearing sharp prominent dental processes on the anterior border, and with a pair of simple tentacula having a dark ocellus at the outer side of the base of each. A well-formed foot arises by a narrow pedicle from the under surface of the body, immediately behind the ciliated collar. The creeping disc is elongated in form, subquadrate in front, and tapers off gradually towards the posterior extremity. The latter part, corresponding to the operculigerous lobe of other species, is speckled with little clusters of dark pigment-cells, disposed so much after the manner of those of the ciliated arms as to lead to the impression that it is one of the same series, or whorl of organs, to use botanical phraseology. A pectinate gill extends beneath the mantle, along the anterior third of the dorsal region, lying, as in most cases, in advance of the heart. The visceral mass of the body, though elongated, is but slightly curved upon itself, not exceeding half a turn. The lobules of the liver, distended with large amber-coloured oil-globules, may be distinctly seen through the transparent outer envelope and shell. Single spherical otolithes are contained in the acoustic sacs, and the lingual ribbon is lengthy and flexuous, presenting a row of uncini on each side, with a series of minute denticulations, pointing backwards on their anterior and posterior borders. The uncini of opposite sides interlock with one another so closely as to conceal the rudimentary segments of the rachis almost completely. The shell is cartilaginous, transparent, planorbicular, and perfectly symmetrical, presenting four rows of minute conical tuberculations on its convex or dorsal surface. The gyri of the involute nucleus are so curved as to leave a central perforation; the mouth of the tube encroaches considerably on the last whorl, and the outer lip is deeply notched between the two lateral rows of tubercles. The author has named the species *Jasonilla M^cLeayiana*. The paper is accompanied with illustrative figures.

V. Note "On the position of Aluminum in the Voltaic series." By Charles Wheatstone, Esq., F.R.S. Received April 25, 1855.

Having, through the kindness of Dr. Hofmann, been permitted to examine a specimen of aluminum prepared by M. Claire-Deville, I

availed myself of the opportunity to ascertain one of the physical properties of this extraordinary metal, which it does not appear has been yet determined, viz. its order in the voltaic series. The following are the results of my experiments.

Solution of potass acts more energetically and with a greater evolution of hydrogen gas upon aluminum than it does on zinc, cadmium or tin. In this liquid aluminum is negative to zinc, and positive to cadmium, tin, lead, iron, copper and platina. Employed as the positive metal, the most steady and energetic current is obtained when it is opposed to copper as the negative metal; all the other metals negative to it which were tried became rapidly polarized, whether above or below copper in the series.

In a solution of hydrochloric acid aluminum is negative to zinc and cadmium, and positive to all the other metals above named. With this liquid also copper opposed to it as the negative metal gave the strongest and most constant current.

Nitric and sulphuric acids are known not to act chemically in any sensible manner on aluminum. With the former acid diluted as the exciting liquid aluminum is negative to zinc, cadmium, tin, lead and iron. The current with zinc is strong; with the other metals very weak, and it is probable that their apparent negative condition is the result of polarization. When aluminum is immersed in dilute sulphuric acid, this metal appears negative to zinc, cadmium, tin and iron, but with lead, on which sulphuric acid has no action, the current is insensible. In both these liquids copper and platina are negative to aluminum, and notwithstanding the apparent absence of chemical action on the latter metal, weak currents are produced.

It is rather remarkable, that a metal, the atomic number of which is so small, and the specific gravity of which is so low, should occupy such a position in the electromotive scale as to be more negative than zinc in the series.

VI. "An Experimental Inquiry into the nature of the metamorphosis of Saccharine Matter, as a normal process of the animal economy." By Frederick W. Pavy, M.D. Lond. Communicated by G. O. Rees, M.D., F.R.S.

This paper was in part read.

May 3, 1855.

CHARLES WHEATSTONE, Esq., V.P., in the Chair.

In accordance with the Statutes, the Secretary read the following list of Candidates recommended by the Council for election into the Society.

Arthur Connell, Esq.
William Farr, Esq.
William Lewis Ferdinand Fischer, Esq.
Isaac Fletcher, Esq.
William John Hamilton, Esq.
John Hawkshaw, Esq.
John Hippisley, Esq.
James Luke, Esq.
A. Follett Osler, Esq.
Thomas Thomson, M.D.
Charles B. Vignoles, Esq.
Charles Vincent Walker, Esq.
Robert Wight, M.D.
Alexander William Williamson, Esq.
George Fergusson Wilson, Esq.

The reading of Dr. Pavy's paper, entitled "An Experimental Inquiry into the nature of the metamorphosis of Saccharine Matter, as a normal process of the animal economy," was resumed and concluded.

The author begins by observing, that the saccharine matter met with in the animal economy is derived from two sources—from the vegetable kingdom, and from the liver of the animal itself; in each case being poured into the general circulation through the hepatic veins. The liver not only enjoys the power of forming sugar, but it likewise exerts (as shown by the experiments of Bernard) some modifying influence over that which is traversing its capillaries and which has been absorbed from the food, by which it is transformed from *vegetable* into *animal* sugar, and thus rendered more apt for serving in the processes of animal life.

The sugar poured into the general circulation through the hepatic veins is conveyed to the capillaries of the lungs, where it in great part disappears, but never entirely so, according to very numerous analyses which the author has made on this subject. If the blood be traced onwards from the arteries through the systemic capillaries into the veins, the small amount of sugar which impregnates arterial blood will be found to be still undergoing a process of destruction; and what appears exceedingly interesting, this process of destruction is not carried on with equal activity in the different parts of the system at large. In the capillaries of the chylo-poietic viscera, the destruction is so complete, that the blood in the portal vein may be entirely free from saccharine principle, when the blood returning from other parts, as that contained in the femoral or jugular veins, remains slightly impregnated. This curious fact has a bearing that will be presently adverted to, with reference to the views to be advanced concerning the nature of the metamorphosis of sugar in the animal economy.

The *principal* seat of destruction of saccharine matter in the animal system being located in the respiratory organs, seems at first sight to support the theory of Liebig—that sugar is one of those substances which undergoes a process of combustion, by its direct combination with oxygen and its resolution into water and carbonic acid. Some experiments on the temporary obstruction of the respiration and the examination of arterial blood before and after the operation, led the author to call in question this view, as he observed that notwithstanding the supply of oxygen was cut off to such an extent as almost to occasion death, yet a considerable destruction of sugar took place in the lungs. This, coupled with the fact that a disappearance of sugar takes place in the systemic capillaries, and unequally so in different portions of them, induced him to push his investigations, and see if there might not be some other cause in operation in the living animal to effect the normal destruction of sugar, besides the direct chemical action of the oxygen absorbed in respiration. The results of these investigations, which were first directed towards the changes produced in blood normally containing sugar, injected through the capillaries of lungs removed from the animal, and artificially inflated with atmospheric air or oxygen gas, have induced the author to refer the metamorphosis of sugar in the

animal economy, to a process which is perfectly consistent and analogous with the well-known chemical bearings of this substance apart from the animal system.

In experiments which the author has now several times repeated, he injected blood removed from the right side of the heart of an animal—and therefore normally containing sugar—through the capillaries of the artificially inflated lungs of another; and found that as long as the blood retains its fibrine, there is as much destruction of its sugar as would take place in the living animal; but that where the fibrine has been separated from the serum and corpuscles, the sugar ceases to be influenced by the presence of oxygen, or ceases to disappear during this process of artificial respiration. It would hence appear, that something besides mere contact with oxygen is requisite for the destruction of sugar. But in other experiments, he has found that oxygen is nevertheless a necessary agent concerned in the process of transformation observed during the arterialization of the blood that has not undergone spontaneous coagulation. It would therefore seem, in fact, that oxygen acts secondarily on the sugar through the medium of the fibrinous constituent of the blood:—that it exerts some changes upon this azotized principle, which are capable of inducing the metamorphosis of sugar.

If we look to the ordinary chemical bearings of saccharine matter apart from the animal system, we find that an azotized substance undergoing the molecular changes of decomposition, placed in contact with sugar readily excites a process of fermentation, and converts it by a mere alteration of the grouping of its elements into another substance, one atom of sugar ($C_{12}H_{12}O_{12}$) being resolved into two atoms of lactic acid ($C_6H_6O_6$). We also find that sugar is not susceptible of oxidation except under the influence of strong chemical reagents. Chemical analogy, therefore, would lead us to look upon the secondary action of oxygen as the more probable process of physiological destruction; especially when we take into consideration, that nowhere do we meet with such a constant series of molecular changes taking place as amongst the azotized constituents of a living animal. In the above-mentioned experiment of injecting fibrinated and defibrinated blood through an artificially inflated lung, when the blood is capable of undergoing the molecular changes

of assimilation on contact with oxygen as in the living animal, the sugar in great part disappears, but so soon as the fibrine is separated by spontaneous coagulation, and the blood has thus lost its vital characteristics, oxygen is no longer capable of exerting any metamorphosing influence on its saccharine ingredients.

If the molecular changes occurring during the decomposition of an azotized substance be capable of converting sugar into lactic acid, why should not the molecular changes occurring during the building-up or elaboration of this same nitrogenized compound effect the same? Indeed, we have seen that the process of destruction is carried on to a certain extent in the systemic capillaries, and more especially in those of the chylo-poietic viscera, where the molecular changes of nutrition are also correspondingly carried on with greater activity than elsewhere. So that analogy and experiment would tend to show that the physiological destruction of sugar is owing to a process similar to fermentation induced by the molecular changes occurring in the nitrogenized constituents of the animal during life. And, in accordance with this, we find lactic acid present in the system, and largely separated from arterial blood by the muscular tissue, and the secerning follicles of the stomach.

As regards the lactic acid fermentation, it is well known that the presence of an alkali favours, whilst that of an acid retards the process. In two experiments on animals, the author injected carbonate of soda and phosphoric acid into the circulating current, and observed in the case of the latter that sugar immediately accumulated in the blood.

The preceding observations refer more especially to the changes that take place in the saccharine ingredient of the blood during life; and the author next proceeds to notice some interesting phenomena observable during the decomposition, and even the spontaneous coagulation of blood containing sugar.

If the blood of an animal normally impregnated with sugar be placed aside, and allowed to undergo spontaneous coagulation, on examining separately the serum and clot on the following day, it will be found, that although the serum may be largely saturated with sugar, the clot is entirely, or almost entirely destitute of it. Now, as the clot is moist and remains to a certain extent infiltrated with the serum from which it has partially separated, it would

appear that even the molecular changes arising from the spontaneous coagulation of the blood are sufficient to effect the destruction of normal animal sugar. And this conclusion is strengthened by the fact, that in diabetic blood (the sugar of which, as would appear from other considerations also, is not so susceptible of metamorphosis as the healthy variety) the sugar does not disappear to a similar extent in the clot.

Under the changes of the decomposition of blood, normal animal glucose is very readily metamorphosed. The rapidity of the metamorphosis depends on the activity of the decomposition of the animal substances present, and when the destruction of the sugar is complete the blood has assumed an *acid reaction.*

This acid reaction of decomposing blood is only observable in that which was previously pretty largely impregnated with sugar. It appears to be owing to the formation of lactic acid. Certainly, it cannot be due to carbonic acid, for the reaction remains after exposure to a boiling temperature.

The disappearance of sugar in the manner just pointed out does not depend on the oxygen of the air, except in so far as this agent is concerned in exciting the decomposition of the azotized constituents of the blood; for the sugar disappears as rapidly when there is a small, as when there is a large amount of surface exposed to the air. But if the air be carefully and completely excluded, no signs of decomposition of the animal parts of the blood are to be observed, and under these circumstances the sugar also remains. The disappearance of sugar is more rapid where the fibrine and corpuscles are present, than when the serum is exposed alone; and in accordance with this, the blood in the one case undergoes decomposition much sooner than in the other—a fact easily intelligible from the greater amount of azotized ingredients present.

If blood normally impregnated with saccharine matter be placed aside until signs of incipient decomposition are observed, and the sugar is beginning to disappear, exposure to a current of oxygen rapidly completes the total disappearance of the saccharine constituent. In this observation we have a further illustration of the analogy that appears to exist, in the nature of the metamorphosis of sugar as a physiological process, and that which takes place chemically under the influence of an azotized compound, whose elemen-

tary particles are in a state of molecular transition. During life, the higher organic constituents of the blood are capable of undergoing the changes of assimilation on exposure to contact with oxygen, and there is a considerable destruction of sugar effected; for a short period after death these azotized constituents remain stationary and uninfluenced by oxygen, and with this, there is a corresponding suspension of the transformation of sugar; but finally, the animal matter of the blood on contact with oxygen, especially during a warm temperature, assumes a state of decomposition, the molecular changes of which again excite the destruction or metamorphosis of saccharine matter.

The sugar *disappears far less rapidly* from diabetic blood under the influence of exposure to the atmosphere, than from healthy right-ventricular blood. From these, and a few other observations which he has as yet been able to make on the blood in Diabetes Mellitus, the author, were he to hazard an opinion on the nature of that obscure disease, would be disposed to say that there appears to be a modification of sugar produced by the liver, which is not susceptible of undergoing the normal process of destruction in the animal system, and which, therefore, accumulating in the blood, is eliminated by the kidneys. The experiments of Bernard have shown that vegetable glucose (grape-sugar) is not susceptible of destruction in the processes of animal life, unless converted into animal glucose by the agency of the liver. Diabetic sugar would therefore seem to bear a resemblance in its physiological relations to vegetable, rather than to animal glucose.

The following communications were in part read:—

I. "Researches on the Partition of Numbers." By Arthur Cayley, Esq., F.R.S. Received April 14, 1855.

The author discusses the following problem:—"To find in how many ways a number q can be made up of the elements a, b, c . ., each element being repeatable an indefinite number of times." The solution depends upon a peculiar decomposition of an algebraical

fraction $\frac{\phi x}{fx}$, where the denominator fx is the product of any number of factors, the same or different of the form $1-x^m$, and upon the expansion by means thereof of the fraction in ascending powers of x. The coefficient of the general term is expressed in terms of circulating functions, such that the sums of certain groups of the coefficients are severally equal to zero; these functions the author calls prime circulators. The investigations show the general form of the analytical expression for the number of partitions, and they also indicate how the values of the coefficients of the prime circulators entering into such expression are to be determined.

II. "Further Researches on the Partition of Numbers." By Arthur Cayley, Esq., F.R.S. Received April 14, 1855. With Postscript. Received April 20, 1855.

The memoir contains a discussion of the problem "to find in how many ways a number q can be made up as a sum of m terms with the elements 0, 1, 2,.... k, each element being repeatable an indefinite number of times." The number q may without loss of generality be taken to be equal to $\frac{1}{2}(km-\alpha)$, and the expression for the number of partitions of this number $\frac{1}{2}(km-\alpha)$ is by a peculiar method reduced to the form coeff. x^m in $\frac{\phi x}{fx}$, where $\frac{\phi x}{fx}$ is an algebraical fraction, the form of which depends on the value of k, *but which does in anywise involve the number m*; the denominator fx is the product of factors of the form $1-x^\theta$, and up to certain limiting values of α the fraction is a proper fraction. The author remarks in conclusion that the researches were made for the sake of their application to the theory developed in his "Second Memoir upon Quantics."

May 10, 1855.

The LORD WROTTESLEY, President, in the Chair.

The reading of Mr. CAYLEY's Papers on the Partition of Numbers was resumed and concluded.

The following communications were read :—

I. "An Experimental Inquiry undertaken with the view of ascertaining whether any *force* is evolved during Muscular Contraction analogous to the *force* evolved in the Fish, Gymnotus, and Torpedo." By HENRY FOSTER BAXTER, Esq. Communicated by Sir B. C. BRODIE, Bart., V.P.R.S. Received April 17, 1855.

After referring to the results obtained by Matteucci by means of the frog, by Du Bois-Reymond by means of the galvanometer, and those of Zantedeschi, Buff, Tyndall, Despretz, Becquerel and Matteucci in reference to Du Bois-Reymond's experiments; in repeating the experiments of Du Bois-Reymond, the author has been led to the following conclusions :—

First. That during muscular contraction in man and in frogs an effect upon the galvanometer may be obtained, indicating the manifestation of an electric current; and

Secondly, That this manifestation of an electric current is due, in a great measure, to secondary reactions, viz. between the animal secretions and the solutions on the one hand, and between the solutions and the surfaces of the platinum electrodes on the other; but that there nevertheless remains a *residual effect*, which we cannot refer to either of these actions, or to those pointed out by Du Bois-Reymond.

The principal reasons upon which the author considers that the effects cannot be entirely referred to secondary actions are these :—

1st. The current due to muscular contraction may be made to over-

come a slight constant current circulating through the galvanometer; and 2ndly, the fact obtained by Matteucci with the galvanoscopic frog would indicate that some force *is* evolved during muscular contraction.

Some differences appear to exist in the results obtained by the author and those of Du Bois-Reymond, viz. the *direction* of the current. Du Bois-Reymond considers it as *direct* in the frog, and *inverse* in man; the author's experiments indicate it to be *direct* in both cases, but he thinks that this difference will be found to be more in appearance than in reality, inasmuch as two distinct questions may have been involved, viz. 1st, whether the *muscular current* is affected during the act of contraction; and 2ndly, whether any force *is* or is *not* evolved during muscular contraction.

As the author wishes the paper to be considered as strictly experimental, and his object being to establish facts, he has endeavoured to avoid everything of a purely controversial character.

II. "On a simple Geometrical construction, giving a very approximate Quadrature of the Circle." By C. M. Willich, Esq. Communicated by Professor Stokes, Sec. R.S. Received April 17, 1855.

Let AB be a quadrant of a circle A, B, C. In the arc BC place a chord BD equal to the radius, so that the arc BD is one of 60°. Bisect BD in E; join AE, and produce the joining line to meet the circumference in F. Then AF differs from the side of a square equal in area to the circle by somewhat less than the one four-thousandth part of that side.

The Society then adjourned to the 24th of May.

May 24, 1855.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read :—

I. "A Second Memoir upon Quantics." By ARTHUR CAYLEY, Esq., F.R.S. Received April 14, 1855.

The memoir is intended as a continuation of the author's introductory memoir upon Quantics (vide Proc. R.S. p. 58, and Phil. Trans. 1854, p. 245); the special subject of the memoir is the theorem referred to in the postscript of the introductory memoir, and the numerous developments arising thereout in relation to the number and form of the covariants of a binary quantic. The author, after some remarks as to the asyzygetic integrals and the irreducible integrals of a system of partial differential equations, and after noticing that the number of irreducible integrals is in general infinite, proceeds to establish the above-mentioned theorem, viz. that a function of any order and degree satisfying the necessary condition as to weight, and such that it is reduced to zero by one of the operations $\{xdy\}-xdy$ and $\{ydx\}-ydx$, is reduced to zero by the other of the two operations, *i. e.* that it is a covariant; and he shows how by means of the theorem the actual calculation of the covariants is to be effected. The theorem gives at once (in terms of symbols P, P′, which denote a number of partitions) expressions for the number of the asyzygetic covariants of a given degree and order, or of a given degree only, of a quantic of any order; this enables the discussion of particular cases, but to obtain more general results it is necessary to transform the expressions for the numbers of partitions by the method explained in the author's "Further Researches on the Partition of Numbers." It appears by the resulting formulæ that the number of the irreducible invariants or covariants does in fact become infinite for quantics of an order sufficiently high; the number of the irreducible invariants

first becomes infinite in the case of a quantic of the order 7; the number of irreducible covariants first becomes infinite in the case of a quantic of the order 5. In particular, the formulæ show that in the case of a quantic of the order 5, or quintic, there are 4 irreducible invariants of the degrees 4, 8, 12 and 18, respectively connected by an equation of the degree 36; and that in the case of a quantic of the order 6, or sextic, there are 5 irreducible invariants of the degrees 2, 4, 6, 10 and 15 respectively connected by an equation of the degree 30; so that the system of the irreducible invariants of a sextic is analogous to that of the irreducible invariants of a quintic. The memoir concludes with a table of the covariants of a quadric, a cubic, and a quartic, and of certain of the covariants of a quintic.

II. "On a Decimal Compass Card." By James M. Share, Esq., Master R.N. Communicated by Rear-Admiral Smyth, Foreign Secretary R.S. Received April 23, 1855.

The mariners' compass-needle having of late years received great improvements, I am of opinion it is high time the card, as at present arranged, should take its place by the side of such things as are superseded by others better adapted to the advancing spirit of the times.

I venture to make an attempt to innovate on an old custom, by suggesting the substitution of a compass card containing thirty-six points of ten degrees each—every degree being one-tenth of a point.

By the use of this card the mariner will avoid the constantly recurring trouble of turning degrees into points, and *vice versâ*.

The ship's course having been worked out in degrees, the deviation and local attraction have but to be applied to adapt it to the decimal steering card, thus rendering the "traverse table for points" no longer necessary to those steering by it; the course N. 35° E. being the same as "north three and a half points east," &c. The same remark applies also to astronomical bearings, azimuths, amplitudes, &c.

Should the decimal card be adopted, the old-fashioned method of

"boxing the compass," which takes young people so long to become familiar with, will be entirely superseded, and I think the sooner such method becomes obsolete the better it will be for the interests of the mariner, for, together with other advantages, the tedious operation of a "day's work" will be divested of half the usual trouble.

When giving a course to the "quarter-master," or "man at the wheel," no mistake, so liable to be the case at present, can well occur; it will merely be necessary to direct him to steer, for instance, "north five points east," or more briefly, "north five east," "south six west," &c. &c.

I recollect an instance of a vessel steering N.W. by N. $\frac{1}{2}$ N., instead of W. by N. $\frac{1}{2}$ N. during thick weather in the Bristol Channel, thus running into danger from the similarity of sound between the courses alluded to.

The practical application of the decimal card would not materially affect the charts previously published, which could have printed compasses containing thirty-six points pasted over the others. Such might be sold by any chart-seller.

III. "On the Theory of the Electric Telegraph." By Professor WILLIAM THOMSON, F.R.S. Received May 3, 1855.

The following investigation was commenced in consequence of a letter received by the author from Prof. Stokes, dated Oct. 16, 1854. It is now communicated to the Royal Society, although only in an incomplete form, as it may serve to indicate some important practical applications of the theory, especially in estimating the dimensions of telegraph wires and cables required for long distances; and the author reserves a more complete development and illustration of the mathematical parts of the investigation for a paper on the conduction of Electricity and Heat through solids, which he intends to lay before the Royal Society on another occasion.

Extract from a letter to Prof. Stokes, *dated Largs, Oct.* 28, 1854.

"Let c be the electro-statical capacity per unit of length of the wire; that is, let c be such that clv is the quantity of electricity required to charge a length l of the wire up to potential v. In a

note communicated as an addition to a paper in the last June Number of the Philosophical Magazine, and I believe at present in the Editor's hands for publication, I proved that the value of c is $\frac{I}{2\log\frac{R'}{R}}$, if I denote the specific inductive capacity of the gutta percha, and R, R′ the radii of its inner and outer cylindrical surfaces.

"Let k denote the galvanic resistance of the wire in absolute electro-statical measure (see a paper 'On the application of the Principle of Mechanical Effect to the Measurement of Electromotive Forces and Galvanic Resistances,' Phil. Mag. Dec. 1851).

"Let γ denote the strength at the time t, of the current (also in electro-statical measure) at a point P of the wire at a distance x from one end which may be called O. Let v denote the potential at the same point P, at the time t.

"The potential at the outside of the gutta percha may be taken as at each instant rigorously zero (the resistance of the water, if the wire be extended as in a submarine telegraph, being certainly incapable of preventing the inductive action from being completed instantaneously round each point of the wire. If the wire be closely coiled, the resistance of the water may possibly produce sensible effects).

"Hence, at the time t, the quantity of electricity on a length dx of the wire at P will be $vcdx$.

"The quantity that leaves it in the time dt will be

$$dt\frac{d\gamma}{dx}dx.$$

"Hence we must have

$$-cdx\frac{dv}{dt}dt=dt\frac{d\gamma}{dx}dx \;\ldots\ldots\ldots\ldots\ldots \quad (1).$$

"But the electromotive force, in electro-static units, at the point P, is

$$-\frac{dv}{dx},$$

and therefore at each instant

$$k\gamma=-\frac{dv}{dx} \;\ldots\ldots\ldots\ldots\ldots\ldots \quad (2).$$

"Eliminating γ from (1) by means of this, we have

$$ck\frac{dv}{dt}=\frac{d^2v}{dx^2} \;\ldots\ldots\ldots\ldots\ldots\ldots \quad (3),$$

which is the equation of electrical excitation in a submarine telegraph-wire, perfectly insulated by its gutta percha covering.

"This equation agrees with the well-known equation of the linear motion of heat in a solid conductor; and various forms of solution which Fourier has given are perfectly adapted for answering practical questions regarding the use of the telegraph-wire. Thus first, suppose the wire infinitely long and communicating with the earth at its infinitely distant end: let the end O be suddenly raised to the potential V (by being put in communication with the positive pole of a galvanic battery of which the negative pole is in communication with the ground, the resistance of the battery being small, say not more than a few yards of the wire); let it be kept at that potential for a time T; and lastly, let it be put in communication with the ground (*i. e.* suddenly reduced to, and ever afterwards kept at, the zero of potential). An elementary expression for the solution of the equation in this case is

$$v=\frac{V}{\pi}\int_0^\infty dn\epsilon^{-zn^{\frac{1}{2}}}\frac{\sin\left[2nt-zn^{\frac{1}{2}}\right]-\sin\left[(t-T)2n-zn^{\frac{1}{2}}\right]}{n} \quad . \quad . \quad (4),$$

where for brevity

$$z=x\sqrt{kc} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)."$$

That this expresses truly the solution with the stated conditions is proved by observing,—1st, that the second member of the equation, (4), is convergent for all positive values of z and vanishes when z is infinitely great; 2ndly, that it fulfils the differential equation (3); and 3rdly, that when $z=0$ it vanishes except for values of t between 0 and T, and for these it is equal to V. It is curious to remark, that we may conclude, by considering the physical circumstances of the problem, that the value of the definite integral in the second member of (4) is zero for all negative values of t, and positive values of z.

"This solution may be put under the following form,

$$v=\frac{2V}{\pi}\int_{t-T}^{t}d\theta\int_0^\infty dn\epsilon^{-zn^{\frac{1}{2}}}\cos(2n\theta-zn^{\frac{1}{2}}) \quad . \quad . \quad . \quad (6),"$$

which is in fact the primary solution as derived from the elementary type $\cos\left(2\pi\frac{it}{T}-z\sqrt{\frac{\pi i}{T}}\right)\epsilon^{-z\sqrt{\frac{\pi i}{T}}}$ given by Fourier in his investigation of periodic variations of terrestrial temperature.

"This, if T be infinitely small, becomes

$$v=\frac{2V}{\pi}T\int_0^\infty dn\epsilon^{-zn^{\frac{1}{2}}}\cos(2nt-zn^{\frac{1}{2}}) \quad . \quad . \quad . \quad . \quad . \quad . \quad (7),$$

which expresses the effect of putting the end O of the wire for an infinitely short time in communication with the battery and immediately after with the ground. It may be reduced at once to finite terms by the evaluation of the integral, which stands as follows:—

when t is positive, $\int_0^\infty dn\epsilon^{-zn^{\frac{1}{2}}}\cos(2nt-zn^{\frac{1}{2}})=\frac{\pi^{\frac{1}{2}}z}{4t^{\frac{3}{2}}}\epsilon^{-\frac{z^2}{4t}}$,

and when t is negative, $\qquad\qquad =0.$

And so we have

$$v=T\frac{Vz}{4\pi^{\frac{1}{2}}t^{\frac{3}{2}}}\epsilon^{-\frac{z^2}{4t}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8),$$

or by (6), when t is not infinitely small,

$$v=\frac{Vz}{2\pi^{\frac{1}{2}}}\int_{t-T}^{t}\frac{d\theta}{\theta^{\frac{3}{2}}}\epsilon^{-\frac{z^2}{4\theta}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9),$$

or which is the same,

$$v=\frac{Vz}{2\pi^{\frac{1}{2}}}\int_0^T\frac{d\theta}{(t-\theta)^{\frac{3}{2}}}\epsilon^{-\frac{z^2}{4(t-\theta)}} \quad . \quad . \quad . \quad . \quad (10).$$

It is to be remarked that in (9) and (10) the limits of the integral must be taken 0 to t (instead of $t-T$ to t, or 0 to T), if it be desired to express the potential at any time t between 0 and T, since the quantity multiplied by $d\theta$ in the second number of (6) vanishes for all negative values of θ.

"These last forms may be obtained synthetically from the following solution, also one of Fourier's elementary solutions:—

$$v=\frac{\epsilon^{-\frac{z^2}{4t}}}{t^{\frac{1}{2}}}\cdot\frac{Q}{\pi^{\frac{1}{2}}}\cdot\sqrt{\frac{k}{c}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (11),$$

which expresses the potential in the wire consequent upon instantaneously communicating a quantity Q of electricity to it at O, and leaving this end insulated. For if we suppose the wire to be continued to an infinite distance on each side of O, and its infinitely distant ends to be in communication with the earth, the same equation will express the consequence of instantly communicating 2Q to the wire at O. Now suppose at the same instant a quantity $-2Q$ to be com-

municated at the point O′ at a distance $\frac{\alpha}{\sqrt{kc}}$ on the negative side of O, the consequent potential at any time t, at a distance $\frac{z}{\sqrt{kc}}$ along the wire from O, will be

$$v=\frac{Q}{\pi^{\frac{1}{2}}}\left\{\frac{\epsilon^{-\frac{z^2}{4t}}}{t^{\frac{1}{2}}}-\frac{\epsilon^{-\frac{(z+\alpha)^2}{4t}}}{t^{\frac{1}{2}}}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (12);$$

and if α be infinitely small, this becomes

$$v=\frac{Q\alpha}{2\pi^{\frac{1}{2}}}\cdot\frac{z\epsilon^{-\frac{z^2}{4t}}}{t^{\frac{3}{2}}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13),$$

which with positive values of z, expresses obviously the effect of communicating the point O with the positive pole for an infinitely short time, and then instantly with the ground.

"The strength of the current at any point of the wire, being equal to $-\frac{1}{k}\cdot\frac{dv}{dx}$, as shown above, in equation (2), will vary proportionally to $\frac{dv}{dx}$ or to $\frac{dv}{dz}$. The time of the maximum electrodynamic effect of impulses such as those expressed by (11) or (13) will be found by determining t, in each case, to make $\frac{dv}{dz}$ a maximum. Thus we find

$$t=\frac{z^2}{6}=\frac{kcx^2}{6}*,$$

as the time at which the maximum electrodynamic effect of connecting the battery for an instant at O, and then leaving this point insulated, is experienced at a distance x.

"In these cases there is no regular 'velocity of transmission.' But, on the other hand, if the potential at O be made to vary regularly according to the simple harmonic law (sin $2nt$), the phases are propagated regularly at the rate $2\sqrt{\frac{n}{kc}}$, as is shown by the well-known solution

$$v=\epsilon^{-zn^{\frac{1}{2}}}\sin(2nt-zn^{\frac{1}{2}}) \quad . \quad . \quad . \quad . \quad . \quad . \quad (14).$$

* We may infer that the retardations of signals are proportional to the squares of the distances, and not to the distances simply; and hence different observers, believing they have found a "velocity of electric propagation," may well have obtained widely discrepant results; and the apparent velocity would, *cæteris paribus*, be the less, the greater the length of wire used in the observation.

The effects of pulses at one end, when the other is in connexion with the ground, and the length finite, will be most conveniently investigated by considering a wire of double length, with equal positive and negative agencies applied at its two extremities. The synthetical method founded on the use of the solution (11) appears perfectly adapted for answering all the practical questions that can be proposed.

"To take into account the effect of imperfect insulation (which appears to have been very sensible in Faraday's experiments), we may assume the gutta-percha to be uniform, and the flow of electricity across it to be proportional to the difference of potential at its outer and inner surfaces. The equation of electrical excitation will then become

$$kc\frac{dv}{dt}=\frac{d^2v}{dx^2}-hv \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15),$$

and if we assume

$$v=\epsilon^{-\frac{h}{kc}t}\phi \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16),$$

we have

$$kc\frac{d\phi}{dt}=\frac{d^2\phi}{dx^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17),$$

an equation, to the treatment of which the preceding investigations are applicable."

Extract from Letter to Prof. Stokes, *dated Largs, Oct.* 30, 1854.

"An application of the theory of the transmission of electricity along a submarine telegraph-wire, shows how the question recently raised as to the practicability of sending distinct signals along such a length as the 2000 or 3000 miles of wire that would be required for America, may be answered. The general investigation will show exactly how much the sharpness of the signals will be worn down* and will show what maximum strength of current through the apparatus, in America, would be produced by a specified battery action on the end in England, with wire of given dimensions, &c.

"The following form of solution of the general equation

$$kc\frac{dv}{dt}=\frac{d^2v}{dx^2}-hv,$$

which is the first given by Fourier, enables us to compare the times

* See the diagram of curves given below.

until a given strength of current shall be obtained, with different dimensions, &c. of wire;—

$$v=\epsilon^{-\frac{ht}{kc}}\,.\,\Sigma A_i \sin\left(\pi\frac{ix}{l}\right)\,.\,\epsilon^{-\frac{i^2\pi^2 t}{kcl^2}}\,.$$

If l denote the length of the wire, and V the potential at the end communicating with the battery, the final distribution of potential in the wire will be expressed by the equation

$$v=V\frac{\epsilon^{(l-x)\sqrt{h}}-\epsilon^{-(l-x)\sqrt{h}}}{\epsilon^{l\sqrt{h}}-\epsilon^{-l\sqrt{h}}},$$

which, when $h=0$, becomes reduced to

$$v=V\left(1-\frac{x}{l}\right),$$

corresponding to the case of perfect insulation. The final maximum strength of current at the remote end is expressed by

$$\gamma=\frac{V}{k\,l}\cdot\frac{2l\sqrt{h}}{\epsilon^{l\sqrt{h}}-\epsilon^{-l\sqrt{h}}},$$

or, when $h=0$, $\qquad \gamma=\dfrac{v}{kl}\cdot$

Hence if we determine A_i so that

$$\Sigma A_i \sin\left(\pi\frac{i\,x}{l}\right)=-V\frac{\epsilon^{(l-x)\sqrt{h}}-\epsilon^{-(l-x)\sqrt{h}}}{\epsilon^{l\sqrt{h}}-\epsilon^{-l\sqrt{h}}}\ \text{ when } x>0 \text{ and } x<l,$$

the equation

$$v=V\frac{\epsilon^{(l-x)\sqrt{h}}-\epsilon^{-(l-x)\sqrt{h}}}{\epsilon^{l\sqrt{h}}-\epsilon^{-l\sqrt{h}}}+\epsilon^{-\frac{ht}{kc}}\,\Sigma A_i \sin\left(\pi\frac{ix}{l}\right)\epsilon^{-\frac{i^2\pi^2 t}{kcl^2}}$$

will express the actual condition of the wire at any time t after one end is put in connexion with the battery, the other being kept in connexion with the ground.

" We may infer that the time required to reach a stated fraction of the maximum strength of current at the remote end will be proportional to kcl^2. We may be *sure* beforehand that the American telegraph will succeed, with a battery sufficient to give a sensible current at the remote end, when kept long enough in action; but the time required for each deflection will be sixteen times as long as would be with a wire a quarter of the length, such, for instance, as in the French submarine telegraph to Sardinia and Africa. One

very important result is, that by increasing the diameter of the wire and of the gutta-percha covering in proportion to the whole length, the distinctness of utterance will be kept constant; for n varies inversely as the square of the diameter, and c (the electro-statical capacity of the unit of length) is unchanged when the diameters of the wire and the covering are altered in the same proportion.

"Hence when the French submarine telegraph is fairly tested, we may make sure of the same degree of success in an American telegraph by increasing all the dimensions of the wire in the ratio of the greatest distance to which it is to extend, to that for which the French one has been tried." It will be an economical problem, easily solved by the ordinary analytical method of maxima and minima, to determine the dimensions of wire and covering which, with stated prices of copper, gutta percha, and iron, will give a stated rapidity of action with the smallest initial expense.

"The solution derived from the type $\frac{\epsilon^{-\frac{x^2}{4t}}}{t^{\frac{1}{2}}}$ may be applied to give the condition of the wire, when one end, E, is kept connected with the ground, and the other, O, is operated on so that its potential may be kept varying according to a given arbitrary function of the time: only this, which I omitted to mention in my last letter, must be attended to: instead of merely considering sources (so to speak) at O and O′ (the latter in an imaginary continuation of the wire), we must suppose sources at O, O_1, O_2, &c., and at O', O_1', O_2', &c. arranged according to the general principle of successive images, so that the potential at E may be zero, and that at O may be uninfluenced by all other sources except the source at O itself. Taking . . . . $O_2, O_1, O, O', O_1', O_2'$. . . . equidistant, we have only to suppose equal sources, each represented by the type

$$\frac{x\epsilon^{-\frac{x^2}{4t}}}{t^{\frac{3}{2}}},$$

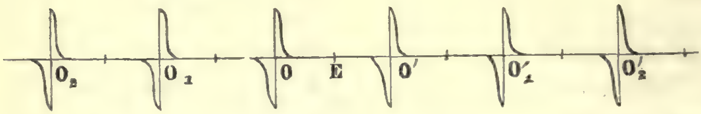
to be placed at these points. For the effects of O_1 and O' will balance one another as far as regards the potential at O.

"So will those of O_2 and O'_1.

,, ,, O_3 and O'_2.

&c. &c.

And again, O and O′ would alone keep the potential at E, zero. So would O_1 and O'_1.
,, O_2 and O'_2.
&c. &c.



Hence if we denote $2lkc$ by a, for brevity, the general solution is

$$v=\frac{1}{2\pi^{\frac{1}{2}}}\int_0^t\frac{d\theta F(\theta)}{(t-\theta)^{\frac{3}{2}}}\left\{\ldots(z+2a)\epsilon^{-\frac{(z+2a)^2}{4(t-\theta)}}+(z+a)\epsilon^{-\frac{(z+a)^2}{4(t-\theta)}}+z\,\epsilon^{-\frac{z^2}{4(t-\theta)}}\right.$$
$$\left.+(z-a)\epsilon^{-\frac{(z-a)^2}{4(t-\theta)}}+(z-2a)\epsilon^{-\frac{(z-2a)^2}{4(t-\theta)}}+\ldots\ldots\right\},$$

where $F(\theta)$ is an arbitrary function such that $F(t)$ expresses the potential sustained at O by the battery.

"The corresponding solution of the equation

$$kc\frac{dv}{dt}=\frac{d^2v}{dx^2}-hv$$

is

$$v=\frac{1}{2\pi^{\frac{1}{2}}}\epsilon^{-\frac{ht}{kc}}\int_0^t\frac{d\theta\epsilon^{\frac{h\theta}{kc}}F\theta}{(t-\theta)^{\frac{3}{2}}}\,\Sigma_{-\infty}^{\infty}\left\{(z-ia)\epsilon^{-\frac{(z-ia)^2}{4(t-\theta)}}\right\},$$

by which the effect of imperfect insulation may be taken into account."

Extract of Letter from Prof. Stokes to Prof. W. Thomson (dated Nov. 1854).

"In working out for myself various forms of the solution of the equation $\frac{dv}{dt}=\frac{d^2v}{dx^2}$ under the conditions $v=0$ when $t=0$ from $x=0$ to $x=\infty$; $v=f(t)$, when $x=0$ from $t=0$ to $t=\infty$, I found that the solution with a single integral only (and there must necessarily be this one) was got out most easily thus:—

"Let v be expanded in a definite integral of the form

$$v=\int_0^\infty\varpi\,(t,\alpha)\sin\alpha x d\alpha,$$

which we know is possible.

"Since v does not vanish when $x=0$, $\frac{d^2v}{dx^2}$ is not obtained by differentiating under the integral sign, but the term $\frac{2}{\pi}\alpha\, v_{x=0}$ must be supplied*, so that (observing that $v_{x=0}=f(t)$ by one of the equations of condition) we have

$$\frac{d^2v}{dx^2}=\int_0^\infty \left\{\frac{2}{\pi}\alpha f(t)-\alpha^2\varpi.\right\}\sin \alpha x dx.$$

Hence

$$\frac{dv}{dt}-\frac{d^2v}{dx^2}=\int_0^\infty \left\{\frac{d\varpi}{dt}+\alpha^2\varpi-\frac{2}{\pi}\alpha f(t)\right\}\sin \alpha x dx,$$

and the second member of the equation being the direct development of the first, which is equal to zero, we must have

$$\frac{d\varpi}{dt}+\alpha^2\varpi-\frac{2}{\pi}\alpha f(t)=0,$$

whence

$$\varpi=\epsilon^{-\alpha^2 t}\int^t \frac{2}{\pi}\alpha f(t)\epsilon^{\alpha^2 t}\, dt,$$

the inferior limit being an arbitrary function of α. But the other equation of condition gives

$$\varpi=\epsilon^{-\alpha^2 t}\int_0^t \frac{2}{\pi}\alpha f(t)\,\epsilon^{\alpha^2 t}\, dt=\left(\frac{\pi}{2}\right)^{-1}\alpha\int_0^t \epsilon^{-\alpha^2\overline{t-t'}} f(t')dt',$$

therefore

$$v=\left(\frac{\pi}{2}\right)^{-1}\int_0^\infty\int_0^t f(t')\alpha\,\epsilon^{-\alpha^2\overline{t-t'}}\sin \alpha x\, d\alpha\, dt'.$$

$$\text{But}\int_0^\infty \epsilon^{-a\alpha^2}\cos b\alpha\, d\alpha=\frac{1}{2}\left(\frac{\pi}{a}\right)^{\frac{1}{2}}\epsilon^{-\frac{b^2}{4a}},$$

therefore

$$\int_0^\infty \epsilon^{-a\alpha^2}\sin b\,\alpha\,.\,\alpha d\alpha=-\frac{d}{db}\left\{\frac{1}{2}\left(\frac{\pi}{a}\right)^{\frac{1}{2}}\epsilon^{-\frac{b^2}{4a}}\right\}$$

$$=\frac{\pi^{\frac{1}{2}}b}{4a^{\frac{3}{2}}}\epsilon^{-\frac{b^2}{4a}},$$

whence writing $t-t'$, x, for a, b, and substituting, we have

$$v=\frac{x}{2\pi^{\frac{1}{2}}}\int_0^t (t-t')^{-\frac{3}{2}}\,\epsilon^{\frac{x^2}{4(t-t')}} f(t')\, dt'.$$

"Your conclusion as to the American wire follows from the dif-

*According to the method explained in a paper "On the Critical Values of the Sums of Periodic Series," Camb. Phil. Trans. vol. viii. p. 533.

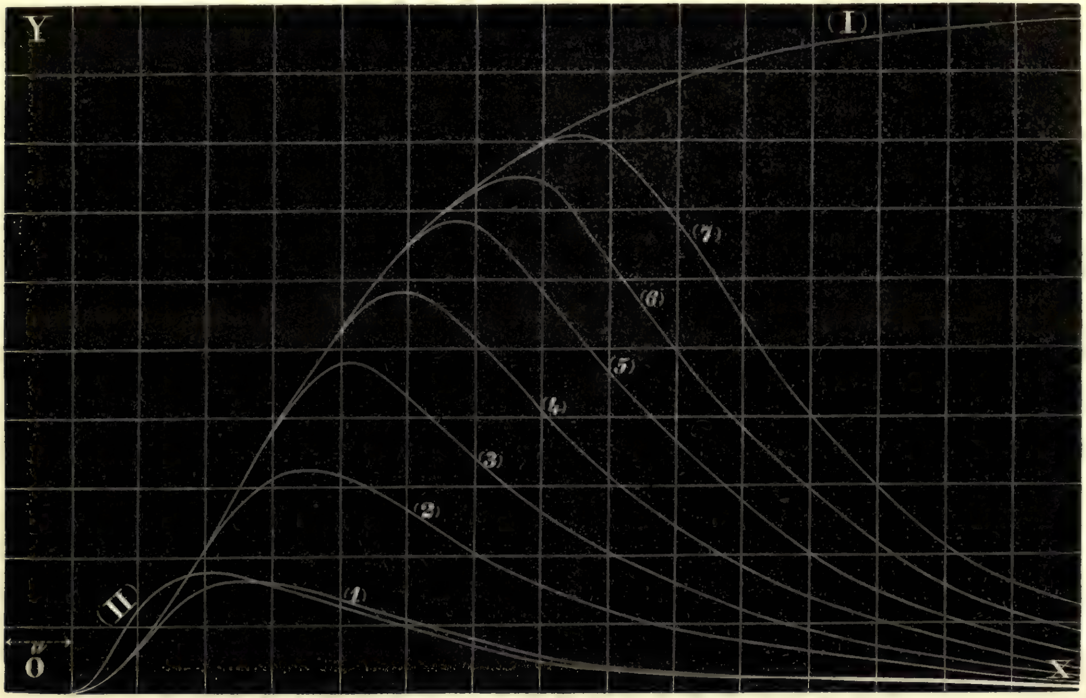
ferential equation itself which you have obtained. For the equation $kc\frac{dv}{dt}=\frac{d^2v}{dx^2}$ shows that two submarine wires will be similar, provided the squares of the lengths x, measured to similarly situated points, and therefore of course those of the whole lengths l, vary as the times divided by ck; or the time of any electrical operation is proportional to kcl^2.

"The equation $kc\frac{dv}{dt}=\frac{d^2v}{dx^2}-hv$ gives $h \propto l^{-2}$ for the additional condition of similarity of leakage."

The accompanying set of curves represents the strength of the current through the instrument at the remote end of a wire as it gradually rises, or gradually rises and falls, after the end operated on is put in connexion with one pole of a battery, and either kept so permanently, or detached and put in connexion with the ground after various short intervals of time.

The abscissas, measured on OX, represent the time reckoned from the first application of the battery, and the ordinates, measured parallel to OY, the strength of the current.

The time corresponding to a is equal to $\frac{kcl^2}{\pi^2}\log_\epsilon\left(\frac{4}{3}\right)$, if l be the length of the wire in feet, k its "resistance" per foot, in electro-statical units, and c its electro-statical capacity per foot (which is equal to $\frac{\mathrm{I}}{2\log\frac{\mathrm{R}'}{\mathrm{R}}}$, if I be the electro-statical inductive power of the gutta percha, probably about 2, and R, R′ the radii of its outer and inner surfaces). The principal curve (I.) represents the rise of the current in the remote instrument, when the end operated on is kept permanently in connexion with the battery. It so nearly coincides with the line of abscissas at first as to indicate no sensible current until the interval of time corresponding to a has elapsed; although, strictly speaking, the effect at the remote end is instantaneous (*i.e.* according to data limited as regards knowledge of electricity, to such as those assumed in hydrodynamics when water is treated as if incompressible, or the velocity of sound in it considered infinitely great, which requires instantaneous effects to be propagated through the whole mass of the water, on a disturbance being made in any part


Y
(I)
(II)
(1)
(2)
(3)
(4)
(5)
(6)
(7)
a
O
X

of it). After the interval a, the current very rapidly rises, and after about $4a$ more, attains to half its full strength. After $10a$ from the commencement, it has attained so nearly its full strength, that the farther increase would be probably insensible. The full strength is theoretically reached only after an infinite time has passed. The first (1) of the smaller curves represents the rise and fall of the current in the remote instrument when the end operated on is put in connexion with the ground after having been for a time a in connexion with the battery; the second (2) represents similarly the effect of the battery for a time $2a$; the third (3) for a time $3a$ and so on. The curve (II) derived from the primary curve (I) by differentiation (exhibiting in fact the steepness of the primary curve at its different points, as regards the line of abscissas), represents the strength of current at different times through the remote end of the wire, consequent upon putting a very intense battery in communication with the end from which the signal is sent, for a very short time, and then instantly putting this end in communication with the ground. Thus, relatively to one another, the curves (1) and (II) may be considered as representing the relative effects of putting a certain battery in communication for the time a, and a battery of ten or twenty times as many cells for a time $\frac{1}{10}a$ or $\frac{1}{20}a$. If I were to guess what might be called "the retardation," which in the observations between Greenwich and Brussels was found to be about $\frac{1}{10}$th of a second, I should say it corresponded to four or five times a, but this must depend on the kind of instrument used, and the mode of making and breaking contacts with the battery which was followed.

Equation of principal curve (I).

$$y=10a-20a(e-e^4+e^9-e^{16}+\&\text{c.}), \text{ where } e=\left(\frac{3}{4}\right)^{\frac{x}{a}};$$

a being half the side of one of the squares.

If $y=f(x)$ denote the equation of the principal wave, and if $f(x)$ be supposed to vanish for all negative values of x, the series of derived curves are represented by the equations

$$(1) \quad . \quad . \quad . \quad . \quad y=f(x)-f(x-a)$$

$$(2) \quad . \quad . \quad . \quad . \quad y=f(x)-f(x-2a)$$

$$(3) \quad . \quad . \quad . \quad . \quad y=f(x)-f(x-3a)$$

$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad .$$

$$(7) \quad . \quad . \quad . \quad . \quad y=f(x)-f(x-7a)$$

$$(\mathrm{II}) \quad . \quad . \quad . \quad . \quad . \quad y=a\frac{df(x)}{da}.$$

I think clearly the right way of making observations on telegraph retardations would be to use either Weber's electro-dynamometer, or any instrument of suitable sensibility constructed on the same principle, that is, adapted to show deflections experienced by a moveable part of a circuit, in virtue of the mutual electro-dynamic force between it and the fixed part of the same circuit due to a current flowing for a very short time through the circuit. Such an instrument, and an ordinary galvanometer, (showing impulsive deflections of a steel needle,) both kept in the circuit at the remote end of the telegraph-wire from that at which the signal is made, would give the values of $\int_0^\infty y^2dx$, and $\int_0^\infty ydx$ (or the area), for any of the curves; and the ratio of the time a of the diagrams to the time during which the battery was held in communication with the wire, might be deduced. The method will lose sensibility if the battery be held too long in communication, but will be quite sufficiently precise if this be not more than ten or twenty times a. I believe there will be no difficulty in applying the method to telegraph-wires of only twenty or thirty miles long, where no retardation would be noticed by ordinary observation. Before, however, planning any observations of this kind with a view to having them executed, I wished to form some estimate of the probable value of a certain element,—the number of electro-statical units in the electro-magnetic unit of electrical quantity,—which I hoped to be able to do from the observation of $\frac{1}{10}$th of a second as the apparent retardation of signals between Greenwich and Brussels. I therefore applied to the Astronomer Royal for some data regarding the mode of observation on the indications of the needle, and the dimensions and circumstances of insulation of the wire; and he was so good as to send me immediately all the information that was available for my purpose. This has enabled me to make the estimate, and so has convinced me that a kind of experiment which I proposed in a paper on Transient Electric Currents in the Philosophical Magazine for June

1853, and which I hope to be able before long to put in practice, will be successful in giving a tolerably accurate comparison of the electro-statical and electro-dynamic units; and, with a further investigation of the specific inductive capacity of gutta percha which will present no difficulty, will enable me to give all the data required for estimating telegraph retardations, without any data from telegraphic operations. This experiment is simply to put two plane-conducting discs in communication with the two poles of a Daniell's battery (or any other battery of which the electromotive force is known in electro-magnetic units), and to *weigh* the attraction between them. I now find that 100 cells of Daniell's so applied would give a force of not less than four grains between two discs each a square foot in area, and placed $\frac{1}{100}$th of a foot apart. As the force varies inversely as the square of the distance between the discs, the weighing will be rather troublesome in consequence of instability, but I think with a good balance it will be quite practicable.

In making this estimate, I suppose the retardation observed between Greenwich and Brussels to be chiefly due to the subterranean part of the wire, and I have taken it as if it were actually observed in 180 miles of coated copper wire. Not having worked out the theoretical problem in the case of a number of insulated wires under the same sheathing, I have considered the cases of a single wire excentrically placed in the iron sheathing, and insulated from it by gutta percha, and a single wire in its own gutta percha tube with the others removed, and itself symmetrically sheathed with tow and iron wire in the usual manner. In the former case the electro-statical capacity of the wire would be $\dfrac{I}{2\log_\varepsilon \dfrac{R^2-f^2}{RR'}}$ approximately*, if R, the inner radius of the conducting sheath, be a considerable multiple of R′ the radius of the copper wire, f denoting he distance between their axes. In the latter case it is $\dfrac{I}{2\log \dfrac{R_{\prime}}{R'}}$, where $R_{\prime}$ is the inner radius of the

* The rigorous expression, which is very easily found by the method of "electrical images," need not be given here.

sheath. These become $\frac{1}{2\cdot45}$ and $\frac{1}{1\cdot35}$, if we take $I=2$ (as it probably is for gutta percha, nearly enough), and $R=\cdot5$, $R_{\prime}=\frac{1}{8}$, $R'=\cdot0325$, as the information given me by the Astronomer Royal indicates. Whatever the theory may show for the influence of the other wires, the result as regards retardation must be intermediate between what it would be if the other wires were removed, and if the one used were separated from them by a sheathing of its own. We may therefore apply the theoretical result by taking c something between $\frac{1}{2\cdot45}$ and $\frac{1}{1\cdot35}$. Hence if "the retardation" agree with the time corresponding to a in the diagrams, k must be intermediate between

$$\frac{\pi^2\times\frac{1}{10}}{\frac{1}{2\cdot45}\times(180\times5280)^2\times\log_\varepsilon\left(\frac{4}{3}\right)} \text{ and } \frac{\pi^2\times\frac{1}{10}}{\frac{1}{1\cdot35}\times(180\times5280)^2\times\log_\varepsilon\left(\frac{4}{3}\right)};$$

or again, if "the retardation" correspond to $9\,a$, k must be intermediate between

$$\frac{\pi^2\times\frac{1}{90}}{\frac{1}{2\cdot45}\times(180\times5280)^2\times\log_\varepsilon\left(\frac{4}{3}\right)} \text{ and } \frac{\pi^2\times\frac{1}{90}}{\frac{1}{1\cdot35}\times(180\times5280)^2\times\log_\varepsilon\left(\frac{4}{3}\right)}.$$

I think it quite certain that what was observed as the retardation must be in reality intermediate between a and $9a$ of the diagrams. Hence the true value of k for 1 foot of the wire must be between the greatest and least of the preceding estimates, that is, between

$$\frac{1}{108\times10^9} \text{ and } \frac{1}{176\times10^{10}}.$$

But the value of K (the "resistance" in British absolute electro-magnetic measure of 1 foot of the wire) must, according to Weber's observations on copper, be about 99810, or nearly enough 100,000*. Hence σ (the number of electro-statical units in the electro-magnetic unit) being equal to $\sqrt{\frac{K}{k}}$, must be between 104,000,000 and 419,000,000.

* See a paper on the application of the general principle of mechanical effect to the theory of electromotive forces, &c., published in the Philosophical Magazine, Dec. 1851.

According to the observations of Weber, Joule, and others, the quantity of water decomposed by a current of unit strength during the unit of time, that is, by the electro-magnetic unit of electricity, is very exactly $\frac{1}{50}$th of a grain. Hence from 2,000,000 to 8,200,000 electro-statical units are required to decompose a grain of water. A positive and a negative electro-statical unit at a foot distance attract one another with a force of $\frac{1}{32\cdot 2}$ of the weight of a grain. Hence if the electricities separated in the decomposition of a grain be concentrated in two points a foot asunder, they will attract with a force of more than 10 tons, and less than 42 tons! Faraday long ago conjectured that less electricity passes in the greatest flash of lightning than in the decomposition of a drop of water, which is now I think rendered very probable.

The expression for the force, in British dynamic units, between two plates, each of area S, at a small distance, a, asunder, when connected with the two poles of a battery of which the electromotive force in electro-magnetic units is F, is $\frac{S}{8\pi}\left(\frac{F}{\sigma a}\right)^2$*, or in terms of the weight of a grain $\frac{1}{32\cdot 2} \cdot \frac{S}{8\pi}\left(\frac{F}{\sigma a}\right)^2$. If F be the electromotive force of 100 cells of Daniell's, which, as I have found from Joule's observations, must be about 250,000,000†, and if a be $\frac{1}{10}$th of a foot, and S a square foot, I conclude from the preceding estimates for σ, that the force of attraction between the plates cannot be less than 4·4 grains, nor more than 72 grains.

It would be easy at any time to make a plan for observing telegraph indications by means of either Weber's electro-dynamometer, or an instrument constructed on the same principle, or by measuring thermal effects of intermittent currents, which could be put in practice by any one somewhat accustomed to make observations, and which would give a tolerably accurate determination of the element of time, even in cases where the observable retardation is considerably less than $\frac{1}{10}$th of a second. A single wire in a submarine cable would, as far as regards the physical deductions to be made from this determination, be to be preferred to one of a number of different wires insulated from one another under the same sheath-

* As was shown at the conclusion of a paper "On Transient Electric Currents," published in June 1853, in the Philosophical Magazine.

† See the paper referred to above, as published in the Phil. Mag., Dec. 1851.

ing. I have little doubt but the Varna and Balaklava wire will be the best yet made for the purpose.

Without knowing exactly what the "retardation" may be in terms of the element of time "*a*" of the diagrams, we may judge what the retardation, if similarly estimated, would be found to be in other cables of stated dimensions. Thus, if the retardation in 200 miles of submarine wire between Greenwich and Brussels be $\frac{1}{10}$th of a second, the retardation in a cable of equal and similar transverse section, extending half round the world (14,000 miles), would be

$$\left(\frac{14000}{200}\right)^2 \times \frac{1}{10} = 490 \text{ seconds, or } 8\tfrac{1}{6} \text{ minutes :}$$

and in the telegraphic cable (400 miles) between Varna and Balaklava, of which the electro-statical capacity per unit of length may be about one-half greater than in the other, while the conducting power of the wire is probably the same, the retardation may be expected to be

$$\left(\frac{400}{200}\right)^2 \times \frac{3}{2} \times \frac{1}{10} = \frac{3}{5} \text{ of a second.}$$

The rate at which distinct signals could be propagated to the remote end would perhaps be one signal in about a quarter of an hour in the former case, and nearly two signals in a second in the latter.

IV. "Observations on the Human Voice." By Manuel Garcia, Esq. Communicated by Dr. Sharpey, Sec. R.S. Received March 22, 1855.

The pages which follow are intended to describe some observations made on the interior of the larynx during the act of singing. The method which I have adopted is very simple. It consists in placing a little mirror, fixed on a long handle suitably bent, in the throat of the person experimented on against the soft palate and uvula. The party ought to turn himself towards the sun, so that the luminous rays falling on the little mirror, may be reflected on the larynx. If the observer experiment on himself, he ought, by means of a second mirror, to receive the rays of the sun, and direct them on the mirror, which is placed against the uvula. We shall

now add our own deductions from the observations which the image reflected by the mirror has afforded us.

Opening of the Glottis.

At the moment when the person draws a deep breath, the epiglottis being raised, we are able to see the following series of movements :—the arytenoid cartilages become separated by a very free lateral movement; the superior ligaments are placed against the ventricles; the inferior ligaments are also drawn back, though in a less degree, into the same cavities; and the glottis, large and wide open, is exhibited so as to show in part the rings of the trachea. But unfortunately, however dexterous we may be in disposing these organs, and even when we are most successful, at least the third part of the anterior of the glottis remains concealed by the epiglottis.

Movement of the Glottis.

As soon as we prepare to produce a sound, the arytenoid cartilages approach each other, and press together by their interior surfaces, and by the anterior apophyses, without leaving any space, or intercartilaginous glottis; sometimes even they come in contact so closely as to cross each other by the tubercles of Santorini. To this movement of the anterior apophyses, that of the ligaments of the glottis corresponds, which detach themselves from the ventricles, come in contact with different degrees of energy, and show themselves at the bottom of the larynx under the form of an ellipse of a yellowish colour. The superior ligaments, together with the aryteno-epiglottidean folds, assist to form the tube which surmounts the glottis; and being the lower and free extremity of that tube, enframe the ellipse, the surface of which they enlarge or diminish according as they enter more or less into the ventricles. These last scarcely retain a trace of their opening. By anticipation, we might say of these cavities, that, as will afterwards appear clearly enough in these pages, they only afford to the two pair of ligaments a space in which they may easily range themselves. When the aryteno-epiglottidean folds contract, they lower the epiglottis, and make the superior orifice of the larynx considerably narrower.

The meeting of the lips of the glottis, naturally proceeding from the front towards the back, if this movement is well managed, it

will allow, between the apophyses, of the formation of a triangular space, or inter-cartilaginous glottis, but one which, however, is closed as soon as the sounds are produced.

After some essays, we perceive that this internal disposition of the larynx is only visible when the epiglottis remains raised. But neither all the registers of the voice, nor all the degrees of intensity, are equally fitted for its taking this position. We soon discover that the brilliant and powerful sounds of the chest-register contract the cavity of the larynx, and close still more its orifice; and, on the contrary, that veiled notes, and notes of moderate power, open both so as to render any observation easy. The falsetto register especially possesses this prerogative, as well as the first notes of the head-voice*. So as to render these facts more precise, we will study in the voice of the tenor the ascending progression of the chest-register, and in the soprano that of the falsetto and head-registers.

Emission of the Chest-voice.

If we emit veiled and feeble sounds, the larynx opens at the notes

do, ré, mi,
2 2 2

†;

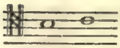
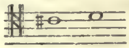
and we see the glottis agitated by large and loose vibrations throughout its entire extent. Its lips comprehend in their length the anterior apophyses of the arytenoid cartilages and the vocal cords; but, I repeat it, there remains no triangular space.

As the sounds ascend, the apophyses, which are slightly rounded on their internal side, by a gradual apposition commencing at the back, encroach on the length of the glottis; and as soon as we

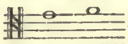
* Let us here observe, that three registers of voice are generally admitted,—chest, falsetto, and head. The first begins lower in a man's voice than in a woman's; the second extends equally in both voices; the third reaches higher in the female voice.

Table of the Human Voice in its full extent.

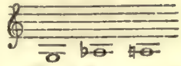
† The musical limits we establish in the course of these pages vary a little in each individual.

reach the sounds si$_3$, do$_3$, , they finish by touching each other throughout their whole extent; but their summits are only solidly fixed one against the other at the notes do♯$_3$, ré$_3$, . In some organs these summits are a little vacillating when they form the posterior end of the glottis, and the two or three half-tones which are formed show a certain want of purity and strength, which is very well known to singers. From the do♯$_3$, ré$_3$,  the vibrations, having become rounder and purer, are accomplished by the vocal ligaments alone, up to the end of the register.

The glottis at this moment presents the aspect of a line slightly swelled towards its middle, the length of which diminishes still more as the voice ascends. We also see that the cavity of the larynx has become very small, and that the superior ligaments have contracted the extent of the ellipse to less than one-half.

When instead of veiled and feeble sounds, we make use of full and vibrating ones, the glottis becomes visible only at the sounds mi$_3$, fa$_3$, , and those above them, a limit which depends to a certain extent on the dexterity of the singer. For all the rest, the organs act as we have just said, but with a double difference: 1. The cavity of the larynx contracts itself more when the voice is intense, than when it is feeble. 2. The superior ligaments are contracted so as to reduce the small diameter of the ellipse to a width of two or three lines. But however powerful these contractions may be, neither the cartilages of Wrisberg, nor the superior ligaments themselves, ever close sufficiently to prevent the passage of the air, or even to render it difficult. This fact, which is verified also with regard to the falsetto and head-registers, suffices to prove that the superior ligaments do not fill a generative part in the formation of the voice. We may draw the same conclusion by considering the position occupied by the somewhat feeble muscles which correspond to these ligaments; they cover externally the extremity of the diverging fibres of the thyro-arytenoid muscles, and take part especially in the contractions of the cavity of the larynx during the formation of the high notes of the chest- and of the head-registers.

Production of the Falsetto.

The low notes of the falsetto,  sol$_2$, la$\flat_2$, la$\natural_2$, show the glottis infinitely better than the unisons of the chest-voice and produce vibrations more extended and more distinct. Its vibrating sides, formed by the anterior apophyses of the arytenoid cartilages, and by the ligaments, become gradually shorter as the voice ascends; at the notes la$_3$, si$_3$, , the apophyses take part only at their summits; and in these notes there results a weakness similar to that which we have remarked in the chest-notes an octave below. At the notes do$\sharp_4$, ré$_4$, , the ligaments alone continue to act; then begins the series of notes called *head-voice.* The moment in which the action of the apophyses ceases, exhibits in the female voice a very sensible difference at once to the ear and in the organ itself. Lastly, we verify, that, up to the highest sounds of the register, the glottis continues to diminish in length and in width.

If we compare the two registers in these movements, we shall find some analogies in them: the sides of the glottis, formed at first by the apophyses and the ligaments, become shorter by degrees, and end by consisting only of the ligaments. The chest-register is divided into two parts, corresponding to these two states of the glottis. The register of falsetto-head presents a complete similarity, and in a still more striking manner.

On other points, on the contrary, these same registers are very unlike. The length of the glottis necessary to form a falsetto note, always exceeds that which produces the unison of the chest. The movements which agitate the sides of the glottis are also augmented and keep the vibrating orifice continually half opened, which naturally produces a great waste of air. A last trait of difference, is in the increased extent of that elliptic surface.

All these circumstances, which we shall refer to again, show in the mechanism of the falsetto, a state of relaxation, which we do not find in the same degree in the chest-register.

Manner in which the sounds are formed.

As we have just said, and what we have seen proves it, the in-

ferior ligaments, at the bottom of the larynx, form exclusively the voice, whatever may be its register or its intensity; for they alone vibrate at the bottom of the larynx *. But by virtue of what principle is the voice formed? It seems to me, that the answer to this question can be but this; the voice is formed in one unique manner,—*by the compressions and expansions of the air, or the successive and regular explosions which it produces in passing through the glottis.*

The ligaments of the glottis are situate about the mean level of the upper border of the cricoid, close the passage, and present a resistance to the air. As soon as the air has accumulated sufficiently, it parts these folds and produces an explosion. But at the same instant, by virtue of their elasticity, and the pressure from below being relieved, they meet again to give rise to a fresh explosion. A series of these compressions and expansions, or of explosions, occasioned by the expansive force of the air and the reaction of the glottis, produces the voice.

This theory, though now generally admitted for reeds, and undoubtedly evident in the liquid vein, the toothed-wheel of Savart, the syrène of the Baron Cagnard Latour &c., has not to my knowledge, been yet applied to the glottis †. If we consider that the lips of this aperture, taken separately, can give no kind of sound, however we may try to make them speak, we must admit that the sounds which they give forth by their mutual action, are only owing to the explosions of the air produced by their strokes ‡. It is not necessary in order to obtain the explosion of sound, that the glottis should be perfectly closed each time after its opening; it suffices that it should oppose an obstacle to the air capable of developing its elasticity. In this case the rushing of the air is heard accompanying

* We gladly acknowledge that this most important fact has been already announced by J. Müller, although we have our objections to the theory which accompanies it.—*Handbuch der Physiologie des Menschen.*

† I find that Dr. Müller hints at the possibility of the voice being thus formed, but only to attack and reject the notion.—*Ibidem.*

‡ Many controversies have arisen respecting the sounds sometimes emitted by animals after the section of the superior and recurrent laryngeal nerves; sounds which have been perhaps occasioned by the struggling of the animal causing a swelling of the neck and a mechanical contact of the vocal ligaments. However, without doubt, after the section of these nerves, voice, as a voluntary act, can no longer take place.

the sounds, and they take a veiled, and sometimes an extremely muffled character; an observation which we have already presented to the reader's notice in speaking of the falsetto.

Conjectures on the Formation of the different Registers.

As the entire system of vibrations arises solely from the inferior ligaments, it is evident that the cause of the different tones called registers, must be sought for in the muscles which set these ligaments in motion; and that the other parts of the larynx must be considered only as apparatus for strengthening the sounds obtained, and for modifying their quality. In our efforts to discover the more intimate processes of the vocal organs which produce the sounds, we shall recur at once to the observations already mentioned, to some anatomical remarks which we are going to make, and to the sensations which we feel in the organ itself whilst it is producing sounds.

If we detach one of the halves of the thyroid cartilage, we shall see a large muscular surface of oblique fibres, which fills all the space between the arytenoid and thyroid cartilages. At its upper end is to be seen the muscle corresponding to the superior vocal ligaments, and which sometimes extends to the notch in the thyroid. After detaching this generally frail muscle, all the fibres constituting this muscular surface seem to start from two opposite centres, viz. the anterior surface of the arytenoid, and the re-entering angle of the thyroid. These centres, occupying the extremities of a diagonal line, send their fibres towards each other in parallel lines. Those which start from the anterior face of the arytenoid descend obliquely; the most external ones go to the cricoid, whose posterior half they cover at the side; the most internal ones descend to the vocal membrane*, which they cover entirely. The fibres which terminate at the membrane become longer, as they become more internal. Those which start from the re-entering angle of the thyroid, reascend obliquely to the summit of the arytenoid, then diverge in order to form the sides of the ventricles, and then disappear in the aryteno-epiglottidean folds and even the under surface of the epiglottis. If we cut it away in successive layers, pro-

* We thus designate that part of the membrane which goes from the bottom of the vocal ligament, to the edge of the cricoid.

ceeding *from* the outside *to* the in, we reach a thick bundle of fibres, perfectly horizontal, which line the outer aspect of the vocal ligament, and which go from the anterior apophyses of the arytenoid to the re-entering angle of the thyroid*.

This bundle has its posterior half covered by the lateral crico-arytenoid muscle, and its anterior half by the diverging fibres which start from the thyroid. If we cut away the horizontal bundle in successive layers, we see that the fibres are not all of the same length; the most external fibres are the longest, and the succeeding ones get gradually shorter as they become more internal; but they all originate in the anterior cavity of the arytenoid, and the muscle is inserted in the manner above explained throughout the whole length of the vocal ligaments, the thyro-arytenoid portion of it excepted. As the fibres all begin from the arytenoid, and terminate successively at more distant points of the membrane, we see that the muscle is thicker behind than before.

Thus the vocal ligament, and the membrane which depends from it, the sole sources of all vocal sounds, are under the direct action of the fibres which come from the anterior cavity of the arytenoid; the ligament under the action of the horizontal bundle, the membrane under that of the oblique fibres. The long horizontal fibres, extending from one cartilage to the other, are placed at the exterior of the short horizontal fibres, and at the interior of the oblique fibres. The diverging fibres which start from the thyroid, acting only on the superior vocal ligaments and the folds, seem to influence by their contractions only the quality and volume of the voice.

The remarkable arrangement of the fibres which we have just examined, enables us to explain a fundamental fact,—the elevation of the voice. The fibres of the horizontal bundle being placed over each other, in layers, one covering the other, and getting gradually longer and longer, as they become more external, extend their action to the more anterior parts of the edges of the glottis. This progressive action from the back to the front, encroaches gradually on the length of the vibrating portion of the ligament, and likewise increases its tension, and its faculty of accelerating its pulsations.

Another portion of the thyro-arytenoid muscle at the same time stretches and raises the vocal membrane more and more, causing a

* Another portion of the thyro-arytenoid muscle.

lesser depth of the ligaments to be in contact, in proportion as the sounds become higher, and thus assists by increasing the mobility of the ligaments.

We shall see in a few moments that the rotatory movement, which the external fibres of the lateral crico-arytenoid muscles give to the arytenoid, by making the vocal membrane deeper, partly counteracts the above effect, and produces the chest-register.

The crico-thyroid muscle, on the contrary, is a powerful auxiliary in the elevation of the voice. This muscle, which at the same time causes the thyroid to come forwards and downwards, gives rise to a mechanical tension, not only in the vocal ligament, but even in the whole vocal membrane. The meeting of the thyroid and cricoid cartilages, which we can feel by the touch, becomes especially marked when the inter-ligamentous glottis alone produces the sounds, which takes place as we have seen at the notes do$_3$♯, ré$_3$ 
in the chest-register, and an octave above for that of the head; with this difference, however, that for the latter a more vigorous and complete connexion is necessary.

Let us now see what we may learn from the sensations we feel in the vocal organ. When we produce a chest-note, the least attention enables us to distinguish a "*pinching*" at the posterior part of the glottis, which becomes more vigorous as the notes ascend. This pinching seems to be formed by extension of the depth of the touching surfaces, and may become very painful; whilst the notes of falsetto, when higher than chest ones, give comparatively great relief to this part, and the surfaces in contact seem to have become thinner.

If we combine these sensations with the different remarks which have been furnished to us by the examination of the muscles, we can fix the particular mechanism of each register.

Chest Register.

In fact, when the arytenoid muscles have brought in contact the arytenoid cartilages, and closed the glottis, the voice may take two very different characters; nay, more, it will be produced in pitches widely apart from one another, and will give forth the chest, or falsetto registers, according as the fibres of the thyro-arytenoid attached to the vocal membrane are active or not. By the action

of these fibres, as we have seen, this muscle raises the vocal membrane, and makes its apposable part thinner; whereas the lateral crico-arytenoid gives a rotatory movement to the cartilage, which brings the apophyses into deep contact. This deep contact, which continues even after the apophyses no longer partake in the vibrations, gives a deep tension to the membranes, increases the depth of their contact*, and, as a necessary consequence, augments the resistance they present to the air. It is to the extent of this resistance that we attribute the formation of the chest-register, so distinct by its particular amplitude. To it we attribute also the slowness of the beats of the glottis, and the consequent low pitch of the sounds, a pitch which, even in the highest tenor voices, is at least an octave lower than the head notes of ordinary soprani.

Register of Falsetto.

When, on the contrary, the external fibres of the lateral crico-arytenoid muscle remain inactive, we produce the falsetto. The lips of the glottis, stretched by the horizontal bundle of the thyro-arytenoid, come in contact by their edge alone, formed at once by the ligament and the apophyses, and offer little resistance to the air. Hence arises the great loss of this agent, and the general weakness of the sounds produced here.

But as soon as we reach the sound do_4, the beats are produced by the ligaments exclusively, and we have attained the head-register. It is certain, as we may deduce from the movement of the ligaments, that then the vocal membrane is raised by the action of the fibres of the thyro-arytenoid muscle, and its surface is diminished to an edge; but we think that the external fibres of the lateral crico-arytenoid, which would prevent this movement, remain inactive. Then also the very decided tension, which the crico-thyroid muscle effects on the vocal tendons, and which accelerates their movements, takes place.

During the chest-register, therefore, the vocal ligaments are stretched, and are in contact to an extent corresponding with the depth of the anterior apophyses of the arytenoid, whilst in the falsetto the edges alone of the ligaments are stretched and apposed; in both cases the sounds being formed, not by the actual vibrations

* It is then that we feel the pinching of which we have spoken.

of either the whole or part of the tendons, but by the successive explosions which they allow.

Pressure of the Air.

Until now, in our remarks on the manner in which the voice is formed, we have only referred to the rigidity of the glottis, a rigidity necessary to accomplish the 1056 vibrations in one second*, which form the do of the chest-voice, and to accomplish the double number which produces the octave above in the head-voice. There is, notwithstanding, another indispensable element for the production of vocal sounds, the pressure of the air. Pressure, as is well known, developes an elastic force in this agent, in a degree inverse to the volume which it occupies. It is by means of this power that the intensity of the sounds is obtained. The intensity of the sound can only depend on the quantity of air which goes to each *sharp* explosion. I say *sharp* explosion, as an express condition: the glottis should close itself perfectly after every vibration; for if the air found a constant passage, as in the notes of falsetto, then the greatest movements of the glottis, and the greatest waste of air, would produce precisely the weakest notes. To reject this theory would be to attribute the intensity of the sound to the extent of the vibrations accomplished by the lips of the glottis, and to suppose that these lips, each taken separately, possess the power of producing sounds, suppositions quite contrary to the facts.

The elastic force of the air arises not only from the compression of the lungs, but also from the contractions of the trachea, which adjusts its calibre to the different dimensions of the glottis. It is by means of this force that the air conquers the continually-increasing obstacle presented by the lips of the glottis when they produce sounds more and more intense.

Thus the problem of the elevation of the voice, always complicated with that of its intensity, in order to be complete, ought to show the connexion which exists between the tension of the lips of the glottis, the pressure of the air, and the number and intensity of the explosions obtained. As a consequence, we may state that the greater pressure of air necessary to produce the greater intensity,

* Pouillet, *Physique*, Sixth Edition, vol. ii. page 77.

would at the same time increase the number of pulsations, and so raise the tone; but to prevent this, the glottis must at the same time be lengthened, and *vice versâ*; or, in other words, that the different lengths of the glottis can, under different degrees of pressure, produce the same number of shocks, but at different degrees of intensity.

Of the Qualities of the Voice.

Various simultaneous causes modify the qualities of the voice:—1, according as the glottis partially or entirely closes the passage between the explosions, it produces veiled or brilliant sounds; 2, the tube which surmounts and surrounds it also greatly affects the quality of the voice; by its contractions it gives brilliancy to it and its widening volume; 3, the epiglottis also plays a very important part, for every time that it lowers itself, and nearly closes the orifice of the larynx, the voice gains in brilliancy; and when, on the other hand, it is drawn up, the voice immediately becomes veiled.

June 7, 1855.

The LORD WROTTESLEY, President, in the Chair.

The Annual General Meeting for the Election of Fellows was held.

The Statutes respecting the election of Fellows having been read, the Rev. Dr. Booth and William Tooke, Esq., were, with the consent of the Society, appointed Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following gentlemen were declared duly elected:—

Arthur Connell, Esq.
William Farr, Esq.
William Lewis Ferdinand Fischer, Esq.
Isaac Fletcher, Esq.
William John Hamilton, Esq.
John Hawkshaw, Esq.
John Hippisley, Esq.
James Luke, Esq.
A. Follett Osler, Esq.
Thomas Thomson, M.D.
Charles B. Vignoles, Esq.
Charles Vincent Walker, Esq.
Robert Wight, M.D.
Alexander William Williamson, Esq.
George Fergusson Wilson, Esq.

The Society then adjourned.

June 14, 1855.

The LORD WROTTESLEY, President, in the Chair.

The following nobleman and gentlemen were admitted into the Society:—

His Grace the Duke of Argyll.
Arthur Connell, Esq.
William Farr, Esq.
William Lewis Ferdinand Fischer, Esq.
Isaac Fletcher, Esq.
John Hawkshaw, Esq.
John Hippisley, Esq.
James Luke, Esq.
Charles B. Vignoles, Esq.
Alexander W. Williamson, Esq

The following gentlemen were recommended by the Council for election as Foreign Members:—

Gustav Lejeune Dirichlet.	Heinrich Rathke.
Julius Plücker.	Carl Rümker.

The President announced that Edward Tuson, Esq., who at last Anniversary had ceased to be a Fellow of the Society in consequence of the non-payment of his subscription, had applied to the Council to be reinstated, alleging that unforeseen circumstances had prevented him from paying the annual contribution. The President therefore, in accordance with the Statutes, gave notice that the question of Mr. Tuson's readmission would be put to the vote at the ensuing ordinary meeting.

The following communications were read:—

I. "Remarks on the Rev. H. Moseley's Theory of the Descent of Glaciers." By JAMES D. FORBES, D.C.L., F.R.S., Corr. Inst. France, and Professor of Natural Philosophy in the University of Edinburgh. Received May 22, 1855.

In a paper "On the Descent of Glaciers," communicated to the Royal Society on the 19th of April, 1855, and printed in their Proceedings, the Rev. Henry Moseley has proposed an explanation of that phenomenon.

The first part of his paper contains a lucid description of the gradual motion of a sheet of lead covering the roof of Bristol Cathedral, which he ascribes (I have no doubt justly) to the successive expansions and contractions of the lead by atmospheric temperature. He explains the influence of the slope of the roof and of the measure of friction upon the motion with his customary precision and clearness. He also finds for the probable measure of the effect or creeping motion of the lead, a quantity which, considering the imperfect nature of the data with regard to temperature, agrees sufficiently well with observation.

In the latter and shorter part of the paper is a transition to the

case of glaciers, whose motion over their beds may, he thinks, be accounted for in the same way, namely, by the alternate contraction and expansion of the ice by diurnal changes of temperature, and he then enters into certain calculations founded principally on data contained in my 'Travels in the Alps of Savoy' in confirmation of this view.

Entertaining as I do the highest respect for Mr. Moseley's eminent attainments as a theoretical mechanician, it is with extreme regret that I find it necessary, in maintenance of the views regarding glacier motion which I have elsewhere advanced, and in the interest of scientific truth, to show (as I believe I can) that Mr. Moseley has been led, apparently by a sudden inadvertency, to uphold an opinion completely indefensible.

I must first object to Mr. Moseley's description or definition of a glacier, as calculated to mislead the inquirer: he says (p. 339), "glaciers are, on an increased scale" [compared to the sheet lead covering of a roof], "sheets of ice placed upon the slopes of mountains." There are certainly some inconsiderable glaciers of the second order to which this description might possibly apply, with the exception of the small thickness inferred by the word "sheet;" but the true glaciers, whose theory has been so often discussed (which theory must evidently likewise include that of glaciers of the second order), cannot fairly be called either *sheets* of ice nor be accurately described as lying *on* the slopes of mountains. They are vast icy accumulations whose depth bears a considerable proportion to their breadth, and which fill mountain ravines or valleys.

Glaciers are very generally hemmed in by precipitous rocks which determine their *contour* or ground plan; they have often to make their way through contracted gorges where the ice occupies (as in the case of the Mer de Glace of Chamouni), within a short distance, a channel but half as wide as it did before. Yet the glacier, preserving its continuity as a whole, expands or contracts in conformity with the irregularities, not only of its lateral walls, but of its bed, forcing itself over obstacles, or even occasionally allowing itself to be cleft into two branches by them, and closing again into a united mass after the insular obstruction has been past. To speak of such resistances of the channel to the progress of the ice as mere *friction*, or of a glacier considered as a solid body and in its whole extent

(or in any considerable part of it) as having an *angle of repose*, as in the case of a substance with a flat base resting on an inclined plane, is evidently inadmissible and tends to mislead. The valley of the Mer de Glace might have almost any possible inclination before the ice would tend to slide out of it *en masse*, for it is moulded to every sinuosity or protuberance of the bed, whether vertical or horizontal. Let Mr. Moseley imagine a sheet of lead having the ground plan of the *Mer de Glace* and confined by margins of wood accurately adapted to it, and he will see that unless lead were so ductile as to be entitled to the appellation of a semifluid, no motion could possibly result, however great might be the slope on which it lay.

I am sorry to find that Mr. Moseley denies entirely (p. 341) the viscous or plastic structure of a glacier as "not consistent with the fact that no viscosity can be traced in its parts when separated." The answer to this objection seems to be merely this; that the viscosity, though it cannot be "traced" in the parts, *if very minute*, nevertheless *exists* there, as unequivocally proved by experiment on the large scale, or even on spaces several yards or fathoms in extent*. The plastic condition of a glacier is, as I have repeatedly stated, no longer an hypothesis, but a *fact*, since I have in many places demonstrated that, account for it as we may, different portions of the same continuous mass of ice are moving at the same moment with different velocities. That a small piece of ice is not sensibly plastic, is not more strange than that the fine blue colour so perceptible in the glacier totally vanishes in its constituent fragments. That *ductility* and *fragility* are not incompatible qualities, is shown by the fact, that sealing-wax at moderate atmospheric temperatures will mould itself (*with time*) to the most delicate inequalities of the surface on which it rests, under a pressure of not more than half an inch of its substance, but may at the same time be shivered to atoms by a blow with a hammer.

The question of plasticity, however, affects only *mediately* Mr. Moseley's theory of the primary cause of motion by dilatation and contraction. According to the views I support, the dilatation and contraction of the ice of glaciers (assuming it to exist) would be inefficient to move the mass unless it moved plastically; and if it moves plastically, the supposition of its thermal expansion is, at all

* See Phil. Trans. 1846, p. 162, and Phil. Magazine (1845), xxvi. p. 414.

events, superfluous, since gravity is in that case a sufficient moving force.

But, it will be argued, if the ice be really acted on by heat and cold as Mr. Moseley supposes, it is a *vera causa* of motion and cannot be neglected. And here we join issue respecting the physical theory proposed.

Mr. Moseley's explanation of the descent of the lead on a roof at an angle much below that at which motion could take place by gravity, friction being allowed for (the *angle of repose*), amounts to this, that every increase of temperature of the mass by the heat of the day expanding it, pushes the lower end downwards *more* than it pushes the upper end upwards; whilst the cold of the night retracts a little the lower end, but (being favoured by the slope) it pulls down the upper end more than it had been pushed up during the heat of the day, and thus by a species of vermicular motion impels the body down the inclined plane. The motion is calculated from a formula including the absolute expansibility of lead, the slope of the roof, the angle of repose, and the *diurnal range* of temperature. Taking then corresponding data for the Mer de Glace of Chamouni, assuming 30° to be the angle of repose of a glacier upon its bed, taking the expansion of ice to be nearly double that of lead (according to experiments made at St. Petersburgh), and the daily range of temperature *of the ice* to be the same as that *of the air* observed by De Saussure on the Col du Géant in the month of July, Mr. Moseley calculates the daily descent of the glacier opposite the Montanvert and compares it with my observations.

Waiving for the moment all other objections, *can we possibly attribute to the ice of the entire mass of this vast glacier an average daily range of temperature of* $4\frac{1}{4}°$ *of Reaumur or* $9\frac{1}{2}°$ *of Fahrenheit?* The idea seems to me to be perfectly untenable.

The expansion and contraction of ice by heat and cold can of course only take place *below* the freezing-point, or 32°. Let it be percolated by water as it may, it cannot rise *above* that temperature nor expand in the smallest degree. But it is a matter perfectly notorious, that, at least in summer, and throughout the whole extent of the Glacier Proper, and even far into the region of the *névé*, the glacier is charged with percolating water derived from superficial fusion. Mr. Moseley admits this, and even attributes the diurnal

oscillation of temperature which he assumes, to the action of water, as in the following passage: "Glaciers are, on an increased scale, sheets of ice placed upon the slopes of mountains, and subjected to atmospheric variations of temperature throughout their masses by variations in the quantity and the temperature of the water, which flowing from the surface everywhere percolates them" (p. 339). This action therefore clearly brings the temperature of the ice up to 32° during the day. But how is the cold of the night to operate in reducing the temperature of a mass of ice certainly from 300 to 600 or more feet in thickness through the enormous average depression of 9½ degrees? The water so efficient by its percolation in *raising* the temperature (if necessary) to 32°, being frozen, is now powerless. Cold can be conveyed downwards, or to speak more correctly, Heat can be transmitted upwards through the ice only by the slow process of conduction, and this on the supposition that the depression of superficial temperature is all that the theory might require. But how stands the fact? Mr. Moseley quotes from De Saussure the following *daily ranges* of the temperature of the air in the month of July at the Col du Géant and at Chamouni, between which points the glacier lies.

At the Col du Géant 4°·257 Reaumur.
At Chamouni ...... 10°·092 Reaumur.

And he assumes "the same mean daily variation of temperature to obtain throughout the length" [and depth?] "of the Glacier du Géant which De Saussure observed in July at the Col du Géant." But between what limits does the temperature of the air oscillate? We find, by referring to the third volume of De Saussure's Travels, that the mean temperature of the coldest hour* (4 A.M.) during his stay at the Col du Géant was 0°·457 Reaumur, or 33°·03 Fahrenheit, and of the warmest (2 P.M.) 4°·714 Reaumur, or 42°·61 Fahrenheit†. So that even upon that exposed ridge, between 2000 and 3000 feet above where the glacier can be properly said to commence, the air does not, on an average of the month of July, reach the freezing-point at any hour of the night. Consequently the *range of temperature attributed to the glacier is between limits absolutely incapable of*

* The observations were made every two hours day and night.

† The corresponding extremes at Chamouni are 53°·25 and 75°·96 Fahr.

effecting the expansion of the ice in the smallest degree. This would of course be still more applicable if we take the mean of the temperatures at Chamouni and the Col du Géant to present the general atmospheric conditions to which the glacier is exposed.

It is in summer that the glacier moves fastest: it is with my observations of motion *in July* that Mr. Moseley compares the results of his theory: and therefore it is of no avail to say that there are periods of the year when congelation penetrates at night some inches, or even it may be some feet into the ice, and when therefore the sensible heat of the glacier may be considered to vary, though, if regard be had to its vast thickness, it must be on an average and in the most extreme circumstances to an absolutely inappreciable degree.

Lastly, Mr. Moseley, whilst condemning in the following passage the theory of glacier motion by the dilatation of water in the interstices of the ice, clearly passes sentence on his own, which could not come into action until the other had already produced its effects: "The theory of Charpentier, which attributes the descent of the glacier to the daily congelation of the water which percolates it, and the expansion of its mass consequent thereon, whilst it assigns a cause which, so far as it operates, cannot, as I have shown, but cause a glacier to descend, appears to me to assign one inadequate to the result; for the congelation of the water which percolates the glacier does not, according to the observations of Professor Forbes, take place at all in summer more than a few inches from the surface. Nevertheless it is in summer that the daily motion of the glacier is greatest." (Moseley, Proc. R.S. vol. vii. p. 341.)

II. "Researches on the Foraminifera.—Part I. General Introduction, and Monograph of the Genus *Orbitolites*." By William B. Carpenter, M.D., F.R.S., F.G.S. &c. Received May 21, 1855.

The group of *Foraminifera* being one as to the structure and physiology of which our knowledge is confessedly very imperfect,

and for the natural classification of which there is consequently no safe basis, the author has undertaken a careful study of some of its chief typical forms, in order to elucidate (so far as may be possible) their history as living beings, and to determine the value of the characters which they present to the systematist. In the present memoir, he details the structure of one of the lowest of these types, *Orbitolites*, with great minuteness; his object having been, not merely to present the *results* of his investigations, but also to exhibit the *method* by which they have been attained; that method essentially consisting in the minute examination and comparison of a *large number* of specimens.

The *Orbitolite* has been chiefly known, until recently, through the abundance of its *fossil* remains in the Eocene beds of the Paris basin; but the author, having been fortunate enough to obtain an extensive series of *recent* specimens, chiefly from the coast of Australia, has applied himself rather to these as his sources of information; especially as the *animals* of some of them have been sufficiently well preserved by immersion in spirits, to permit their characters to be well made out.

As might have been anticipated from our knowledge of their congeners, these animals belong to the *Rhizopodous* type; the soft body consisting of *sarcode*, without digestive cavity or organs of any kind; and being made up of a number of segments, equal and similar to each other, which are arranged in concentric zones round a central nucleus. This body is invested by a calcareous shell, in the substance of which no minute structure can be discerned, but which has the form of a circular disk, marked on the surface by concentric zones of closed cells, and having minute pores at the margin. Starting from the central nucleus,—which consists of a pear-shaped mass of sarcode, nearly surrounded by a larger mass connected with it by a peduncle,—the development of the Orbitolite may take place either upon a *simple*, or upon a *complex* type. In the former (which is indicated by the *circular* or *oval* form of the cells which show themselves at the surfaces of the disk, and by the *singleness* of the row of marginal pores), each zone consists of but a single layer of segments, connected together by a single annular stolon of sarcode; and the nucleus is connected with the first zone, and each zone with that which surrounds it, by radiating peduncles proceeding from this

annulus, which, when issuing from the peripheral zone, will pass outwards through the marginal pores, probably in the form of *pseudopodia*. In the *complex* type, on the other hand (which is indicated by the *narrow* and *straight-sided* form of the superficial cells, and by the *multiplication* of the horizontal rows of marginal pores), the segments of the concentric zones are elongated into vertical columns with imperfect constrictions at intervals; instead of a single annular stolon, there are two, one at either end of these columns, between which, moreover, there are usually other lateral communications; whilst the radiating peduncles, which connect one zone with another, are also multiplied, so as to lie in several planes. Moreover, between each annular stolon and the neighbouring surface of the disk, there is a layer of superficial segments, distinct from the vertical columns, but connected with the annular stolons; these occupy the narrow elongated cells just mentioned, which constitute two *superficial* layers in the disks of this type, between which is the *intermediate* layer occupied by the columnar segments.

These two types seem to be so completely dissimilar, that they could scarcely have been supposed to belong to the same species; but the examination of a large number of specimens shows, that although one is often developed to a considerable size upon the simple type, whilst another commences even from the centre upon the complex type, yet that many individuals which begin life, and form an indefinite number of annuli, upon the simple type, then take on the more complex mode of development.

The author then points out what may be gathered from observation and from deduction respecting the *Nutrition* and mode of *Growth* of these creatures. He shows that the former is probably accomplished, as in other Rhizopods, by the entanglement and drawing in of minute vegetable particles, through the instrumentality of the pseudopodia; and that the addition of new zones probably takes place by the extension of the sarcode through the marginal pores, so as to form a complete annulus, thickened at intervals into segments, and narrowed between these into connecting stolons, the shell being probably produced by the calcification of their outer portions. And this view he supports by the results of the examination of a number of specimens, in which *reparation of injuries* has taken place. Regarding the *Reproduction* of Orbitolites, he is only able

to suggest that certain minute spherical masses of sarcode, with which some of the cells are filled, may be *gemmules*; and that other bodies, enclosed in firm envelopes, which he has more rarely met with, but which seem to break their way out of the superficial cells, may be *ova*. But on this part of the inquiry, nothing save observation of the animals in their living state can give satisfactory results.

The regular type of structure just described is subject to numerous *variations*, into a minute description of which the author next enters; the general results being, that neither the shape nor dimensions of the entire disk, the size of the nucleus or of the cells forming the concentric zones, the surface-markings indicating the shape of the superficial cells, nor the early mode of growth (which, though typically *cyclical*, sometimes approximates to a *spiral*), can serve as distinctive characters of *species*; since, whilst they are all found to present most remarkable differences, these differences, being strictly gradational, can only be considered as distinguishing *individuals*. It thus follows that a very wide *range of variation* exists in this type; so that numerous forms which would be unhesitatingly accounted specifically different, if only *the most divergent examples* were brought into comparison, are found, by the discovery of those *intermediate links* which a large collection can alone supply, to belong to one and the same specific type.

After noticing some curious *monstrosities*, resulting from an unusual outgrowth of the central nucleus, the author proceeds to inquire into the *essential character* of the Orbitolite, and its relations to other types of structure. He places it among the very lowest forms of Foraminifera; and considers that it approximates closely to sponges, some of which have skeletons not very unlike the calcareous net-work which intervenes between its fleshy segments. Of the *species* which the genus has been reputed to include, he states that a large proportion really belong to the genus *Orbitoides*, whilst others are but varieties of the ordinary type. This last is the light in which he would regard the *Orbitolites complanata* of the Paris basin; which differs from the fully-developed Orbitolite of the Australian coast in some very peculiar features (marking a less complete evolution), which are occasionally met with among recent forms, and which are sometimes distinctly transitional towards the perfect type.

The author concludes by calling attention to some general principles, which arise out of the present inquiry, but which are applicable to all departments of Natural History, regarding the *kind* and *extent* of comparison on which alone specific distinctions can be securely based.

June 21, 1855.

The LORD WROTTESLEY, President, in the Chair.

A. Follett Osler, Esq., Charles Vincent Walker, Esq., and Robert Wight, M.D., were admitted into the Society.

The following gentlemen were elected Foreign Members of the Society:—

Gustav Lejeune Dirichlet.
Julius Plücker.
Heinrich Rathke.
Carl Rümker.

Pursuant to notice given at last Ordinary Meeting, the question of the readmission of Edward Tuson, Esq., was put, and, the ballot having been taken, decided in the negative.

The following communications were read:—

I. "On a supposed Aërolite or Meteorite found in the Trunk of an old Willow Tree in the Battersea Fields." By Sir Roderick Impey Murchison, F.R.S., Director-General of the Geological Survey of Great Britain. Received June 21, 1855.

In bringing this notice before the Royal Society, it is unnecessary to recite, however briefly, the history of the fall of aërolites or meteorites, as recorded for upwards of three thousand years, though I may be

pardoned for reminding my Associates, that the phenomenon was repudiated by the most learned academies of Europe up to the close of the last century. The merit of having first endeavoured to demonstrate the true character of these extraneous bodies is mainly due to the German Chladni (1794), but his efforts were at first viewed with incredulity. According to Vauquelin and other men of eminence who have reasoned on the phenomena, it was in 1802 only that meteorites obtained a due degree of consideration and something like a definite place in science through the studies of Howard, as shown in his memoir published in the Philosophical Transactions.

Vauquelin, Klaproth, and other distinguished chemists, including Berzelius and Rammelsberg, have successively analysed these bodies, and the result of their labours, as ably brought together in the work of the last-mentioned author, is, that whilst they have a great general resemblance and are distinguishable on the whole by their composition from any bodies found in the crust of the earth, each of their component substances is individually found in our planet. They are also peculiarly marked by the small number of minerals which have collectively been detected in any one of them; nickel and cobalt, in certain relations to iron, being the chief characteristics of the metallic meteorites.

Of the various theories propounded to account for the origin of these singular bodies, it would indeed ill become a geologist like myself to speak; and referring in the sequel to some of the various works in which the subject has been brought within formula, I will at once detail the facts connected with the discovery of this metalliferous body in the heart of a tree, as now placed before the Members of our Society, feeling assured that, whatever be their ultimate decision, my contemporaries will approve of the efforts that have been made to account for this singular and mysterious phenomenon.

On the 2nd of June, a timber merchant, residing at North Brixton, named Clement Poole, brought the specimen now exhibited to the Museum of Practical Geology, when it occurred to Mr. Trenham Reeks, our Curator, that it might be a meteorite, and on inspecting its position in the mass of wood, and having heard all the evidence connected with it, I was disposed to form the same conclusion. On submitting a small portion of the metallic part to a qualitative test in the metallurgical laboratory of our establishment, the presence of

nickel, cobalt and manganese was detected in the iron included in the mass, and as the surface was scorified, indented, uneven, and partially coated with a peculiar substance, the surmise as to the meteoric nature of the imbedded material seemed to be rendered much more probable. Again, in looking at the wood which immediately surrounded that portion of the mass which remained, as it is now, firmly inserted in the tree, a blackened substance was observed to be interpolated between the supposed meteorite and the surrounding sound wood. On the outside of this substance (which had somewhat a charred aspect) we observed a true bark, which follows the sinuosities of the wood wherever the latter appears to have been influenced by the intrusion of the foreign mineral matter. [The specimen is represented in the annexed wood-cut.]

Seeing thus enough to satisfy our conjecture, if sanctioned by other evidence, I desired Mr. Poole to bring all the fragments of the wood he had not destroyed which surrounded this body. On placing the ends of some of these (also now exhibited) on the parts from which they had been sawed off, they indicated that the space between the mineral substance and the surrounding sound wood widened upwards; the decayed wood passing into brown earthy

matter with an opening or cavity into which rootlets extended. On interrogating Mr. Poole, who cut down the tree and superintended the breaking up of its timber, I learnt from him all requisite particulars respecting its dimensions, the position of the ferruginous mass, the quantity of wood above and below it, a description of the place where the stool of the tree was still to be seen, and of the parties who, living on the spot, were acquainted with every circumstance which could throw light on the case.

At this period of the inquiry, the Museum in Jermyn Street was visited by Dr. Shepard, Professor in the University College, Amherst, United States, whose researches on meteorites are widely known, and who has furnished an able classification of them by which they are divided into the two great classes of stony and metallic. Having carefully examined the specimen, Dr. Shepard expressed his decided belief that it was a true meteorite, and the next day wrote to me the following account of it; at the same time referring me most obligingly to a series of interesting publications on the subject as printed in America and Europe*:—

"Concerning the highly interesting mineral mass, lately found enclosed in the trunk of a tree, and of which you have done me the honour to ask my opinion, I beg leave to observe, that I have no hesitation in pronouncing it to be a true meteoric stone.

"Aside from the difficulty of otherwise accounting for it, under the circumstances in which it is found, the mass presents those

* Dr. Shepard's numerous memoirs on meteorites are all to be found in the volumes of the American Journal of Science and Art, and in the same work the reader will find not only the general classification of these bodies by this author, who possesses a collection from 103 localities, but also essays on the same subject by his countrymen Dr. Troost, Professor Silliman, jun., and Dr. Clark.

In our own country, Mr. Brayley published some years ago a comprehensive view of this subject in the Philosophical Magazine, and recently Mr. Greg has in the same publication put together all the previous and additional materials, with tables showing the geographical distribution of meteorites. Among the well-recorded examples of the fall of metalliferous meteorites, no one is more remarkable than that which happened in the year 1851, about sixteen leagues S.E. of Barcelona in Spain. In describing that phenomenon, Dr. Joaquim Balcells, Professor of Natural Sciences at Barcelona, has illustrated the subject with much erudition, whilst his theoretical views are ingenious in his endeavour to explain how meteorites are derived from the moon.

peculiar traits that are regarded as characteristic of meteorites. It has, for example, a fused, vitrified black coating, which is quite continuous over a considerable part of the mass, and contains several grains and imbedded nodular and vein-like portions of metallic iron, in which I understand nickel and cobalt have been detected.

"The general character of the body of the stone is indeed peculiar; and as a whole, unlike any one I have yet seen; it being principally made up of a dull greyish yellow, peridotic mineral, which I have nowhere met with among these productions, except in the Hommoney Creek meteoric iron mass, and which exists in it only in a very limited quantity. It is singular to remark also, that the stone under notice strikingly resembles in size, shape and surface, the iron above alluded to.

"The absence of the black, slaggy coating on one of the broad surfaces of the stone, may arise from its having been broken away, by the violence to which it must have been subjected in entering the tree; for it appears to have buried itself completely at its contact, an operation which would probably have been impossible, in the case of a stone, but for its wedge-shaped configuration, and the coincidence of one of its edges with the vertical fibres of the wood."

In reply to a question I subsequently put to Dr. Shepard as to whether he knew of any examples of meteorites having struck trees in America, he replied as follows:—

"I think you will find in the volume I left with Mr. Reeks at the Museum, an account of the fall of Little Piney, Missouri, February 13th, 1839; in which it is stated that the stone struck a tree and was shattered to fragments, it being one of a brittle character. In the interior of the Cabarras county, N. Carolina, a stone (October 31, 1849) I know struck a tree, and I found it was difficult, indeed impossible, to separate completely the adhering woody fibres from the rough hard crust of the meteorite. The stone in this case is a peculiarly tough one, having a decidedly trappean character, rendering it as nearly infragile as cast iron."

Aware that some time must elapse before the precise analysis, which I wished to be made in the laboratory of Dr. Percy, could be completed, and that the last meeting of the Royal Society was to be held this evening, I announced the notice I am now communicating. At the same time I resolved to visit the locality where the tree stood,

and to obtain on the spot all the details required. Having done so, accompanied by Mr. Robert Brown, Sir Philip Grey Egerton, Professor J. Nicol, and Mr. Trenham Reeks, the information ultimately obtained was as follows:—

The man who helped to cut down the tree confirmed in every respect the evidence of Mr. Poole as to its position, height, and dimensions, and pointed out to us the stump or stool we were in search of, which is to be seen at nearly 200 yards to the east of the St. George's Chapel, Lower Road, Battersea Fields, and at the eastern end of a nursery garden, between the railway and the road, occupied by Mr. Henry Shailer.

The tree was a large willow, probably about sixty years of age, which stood immediately to the east of the old parsonage house recently pulled down. Its stem measured about 10 feet in circumference at 3 feet above the ground, and had a length of between 9 and 10 feet; from its summit three main branches extended, one of which, pointing to the S.W. or W.S.W., had been for many years blighted, and was rotten to near its junction with the top of the main trunk; a portion of this blighted main branch is exhibited. The other two main branches, which rose to a height of 50 or 60 feet, were quite sound; a part of one of these offsets is also exhibited.

The stool of the tree was visibly perfect and without a flaw, and at the wish of Mr. R. Brown, a section of it has been obtained since our visit, which is also here, and the rings of which seem to confirm the supposition as to the age of the tree.

Mr. Poole having conveyed the tree to Brixton, cut the trunk into two nearly equal parts, intending to make cricket-bats out of each. In doing so, he perceived that the upper portion of the lower of the two segments was in a shaky or imperfect condition, and hence he resolved to saw off the upper part of it, intending thereby to obtain wood large enough for the "pods" of his cricket-bats, but not such entire bats as he was making out of the upper segment.

In dividing the tree, the saw was stopped at about 8 inches from the surface on one side (or the breadth of a large saw) by a very hard, impenetrable substance, which was supposed to be a nail, and hence Mr. Poole resolved to break up the portion of the wood he had previously condemned as of inferior quality, and hewing it down

from the sides he uncovered, to his astonishment, the great lump of metalliferous matter, as now seen. Attaching little value to it, much of the surrounding wood was thrown away or used up before the specimen was brought to Jermyn Street; but enough has been obtained to throw light on the probable or possible origin of the included mass.

On interrogating Henry Shailer, a market gardener, who has long lived on the spot and managed the ground where the tree grew, when it was part of the garden of the former clergyman (Mr. Weddell), I learnt from him that he had known the spot for sixty years, that in his days of boyhood it was a fellmonger's yard, before it was attached to the garden. He had observed that the tree was blighted in one of its main branches for many years, and had always supposed that it was struck by lightning in one of two storms, the first of which happened about 1838 or 1839, the other about nine years ago.

So far the evidence obtained might be supposed to favour the theory that this ferruginous mass* had been discharged near to the blighted branch, and had penetrated downwards into the tree, to the position in which we now see it, charring and warping the wood immediately around it in its downward progress; whilst in the sixteen years which have elapsed, the wood renovating itself, produced the appearance which has so much interested the eminent botanists who have examined it, viz. Mr. R. Brown, Dr. Lindley, Professor Henfrey, Dr. J. Hooker, and Mr. Bennett.

On the other hand, I must now point out some features of this extraordinary case which check the belief in the included mass being a meteorite.

We found lying near the root of the tree two fragments, one of which is similar to the substance included in the tree, while the other is decidedly an iron slag. On bringing these fragments, weighing several pounds, to Jermyn Street, and on breaking one of them, it was found, like the supposed meteorite, to contain certain small portions of metallic iron, in which both nickel and cobalt were also present; and hence the scepticism which had prevailed from

* The ferruginous mass is, it is supposed, about thirty pounds in weight; but as one of its extremities is still imbedded in the wood, the precise weight cannot be stated.

the beginning of the inquiry in the minds of some of my friends, was worked up into a definite shape.

The occurrence of stones enclosed in wood is not a novel phenomenon. Mr. Robert Brown has called my attention to two cases as recorded in the following works:—

"De lapide in trunco betulæ reperto. G.F. Richter in Acta Phys. Med. Acad. Nat. Curios. volume 3, page 66*."

"Descriptio Saxi in Quercu inventi. Kellander, Acta Literaria et Scientiæ Sueciæ." 1739, pp. 502, 503.

Since the Battersea phenomenon was announced, Professor Henslow, to whom I had applied, wrote to me saying, that he possessed a remarkable example of a stone which was found imbedded in the heart of a tree, in sawing it up in Plymouth Dockyard; and he has obligingly sent up the specimen, which is now also exhibited. In this case, judging from the mineral character of the rock, and its being slightly magnetic, Professor Henslow supposed that it was perhaps a volcanic bomb. On referring it to Dr. Shepard, that gentleman entertains the opinion that it is also a meteorite, and states that it resembles certain meteoric stones with which he is acquainted; suspicions of which had also been entertained by Professor Henslow, From the examination of a minute fragment which I detached from this stone, it appears to be composed of a base of felspathic matter, with minute crystals of felspar and of magnetic iron pyrites. Externally it has a trachytic aspect, though, when fractured, it more resembles, in the opinion of Mr. Warington Smyth, a pale Cornish elvan or porphyry than any other British rock with which it can be compared. Whatever may have been the origin of this stone, which is of the size of a child's head, it is essentially different from the metalliferous mass from Battersea, to which attention has been specially invited, and its position in the heart of an oak is equally remarkable. Like the Battersea specimen, the segment of wood from Plymouth Dockyard is characterized by an interior bark which folds round the sinuosities of the included stone.

In respect to the envelopment of manufactured materials in trees,

* "Lapis prædurus subalbicans et manifeste siliceus pruni ferme aut juglandis minoris magnitudine. * * * * Nidus ad figuram lapidis non plane accommodatus, sed quadrangulus, et hinc illinc in mediocres rimas desinens, corticeque imprimis notabili, non multum ab exteriori cute diverso, maximam partem vestitus."

my friend, Mr. H. Brooke, the distinguished mineralogist, tells me that he perfectly remembers the case of an iron chain which had been enclosed in the heart of a tree, the wood of which was sound around the whole of the included metallic body. This specimen was to be seen some years ago in the British Museum. Again, he informs me that at Stoke Newington he recollects to have seen a tree, the trunk of which had grown over and completely enclosed a scythe, except on the sides where its ends protruded*.

Whatever may have been the origin of the metalliferous mass from Battersea, its discovery has at all events served to develope certain peculiarities in the growth of plants which appear to be of high interest to the eminent botanists who have examined the parts of this tree which surrounded the supposed meteorite. Unwilling to endeavour to anticipate the final decision as to the origin of the body in question, I may be permitted to feel a satisfaction that its discoverer brought it to the Establishment of which I am the Director, and which numbers among its officers a Fellow of this Society, who is so well calculated, by his analytical researches, to settle the question on a permanent basis. Should the metallurgical analyses now under the conduct of Dr. Percy lead to the inevitable conclusion that the composition of this body is different from that of well-authenticated meteorites, and is similar to that of undoubted iron slags, we shall then have obtained proofs of the great circumspection required before we assign a meteoric origin to some of these crystalline iron masses, which though not seen to fall, have, from their containing nickel, cobalt and other elements, been supposed to be formed by causes extraneous to our planet.

Postscript, 30th June 1855.—The following are the analyses above referred to, which have been given to me by Dr. Percy since the preceding notice was read:—

"*The slag-like matter* (1) attached to the metal in the tree, as well as the similar matter (2) with adherent metal which was found by

* Many other examples of extraneous bodies found enclosed in the heart of trees have been brought to my notice since this account was written. The most curious of these is perhaps that of an image of the Virgin, which having been placed in a niche had become imbedded by the growth of the tree around it.

Mr Reeks in the vicinity of the tree, has been analysed. The results are as follow :—

	No. 1.	No. 2.
Silica	58·70	63·52
Protoxide of iron	35·46	32·30
Lime	0·30	0·59
Magnesia	0·74	0·21
Protoxide of manganese	trace	trace
Alumina	3·40	2·85
Phosphoric acid	0·43	0·57
Sulphur as sulphide	trace	trace
	99·03	100·04

"No. 1. was analysed by Mr. Spiller, and No. 2. by Mr. A. Dick, chemists who have been incessantly engaged at the Museum during the last two years and a half in the analyses of the iron ores of this country, and whose great experience renders their results worthy of entire confidence. Cobalt and nickel were not sought for in either case, but the metallic iron enveloped in both specimens contained a minute quantity of cobalt and nickel. Another piece of slag-like matter, which was found on the ground near the tree, and which from its external characters I have no hesitation in pronouncing to be a slag, was examined for cobalt and nickel, and gave unequivocal evidence of the former in minute quantity, though not satisfactorily of the latter.

"The metal previously mentioned is malleable iron. That which was detached from the slag-like matter, found outside the tree, was filed and polished, and then treated with dilute sulphuric acid. After this treatment, the surface presented small, confused, irregularly-defined crystalline plates, and was identical in appearance with the surface of a piece of malleable iron similarly treated after fusion in a crucible."

II. "On the Magnetism of Iron Ships, and its accordance with Theory, as determined externally, in recent Experiments." By the Rev. W. Scoresby, D.D., F.R.S., Corr. Memb. Inst. France, &c. Received June 21, 1855.

The magnetic condition of iron ships is a subject of so much importance, practically and scientifically, that I have been induced to submit to the Society a few characteristic facts (hastily indeed brought together) derived from recent experiments.

In a work in the Society's library, entitled 'Magnetical Investigations,' it was shown, by deductions from an elaborate series of experiments on plates and bars of malleable iron, that the magnetic condition of iron ships should, theoretically, be conformable to the direction of terrestrial induction whilst on the stocks; and the *retentive* quality, which is so highly developed by virtue of the hammering and other mechanical action during the building, should be so far fixed in the same direction, as to remain after the ships might be launched, until disturbed by fresh mechanical action in new positions of their head or keel. In this view, taking, for instance, the condition of the middle, or the main breadth section of a ship on the stocks, the magnetic polar axis should assume the direction of the dipping-needle (with an equatorial plane, or plane of no-attraction at right angles to it), passing through or proximate to the centre of gravity of the iron material in such section. Thus every ship should have a characteristic magnetic distribution, primarily, dependent on her position whilst on the stocks; so that, being built with the head north or south, the *equatorial plane* should appear externally on the same horizontal level, the *polar axis* only being inclined from the vertical, in correspondence with the direction of the axis of terrestrial magnetism (Magnetical Investigations, vol. ii. pp. 331, 332).

It was also inferred, that whilst such individuality of the magnetic distribution would be rendered *retentive* on the same principles as this quality of magnetism is developed in bars or plates of iron by mechanical action, so the axial direction of the ship's magnetism would be *liable to change*, under mechanical action, in new positions of the ship's head, or under new relations of terrestrial magnetism,

just as the retentive magnetism is liable to change in bars or plates of iron if hammered, vibrated, or bent whilst held in new positions.

And it was further inferred, that the analogy with plates and bars might be expected to hold, notwithstanding the numerous pieces of which the ship's hull might be composed, because, in experiments on combining short magnets into long series, or submitting piles of short bars of iron to terrestrial induction, it was found that no material difference in the effects occurred betwixt a *single* steel magnet of a given length, or a *single* bar of iron, and the same substance and dimensions combined in short or small pieces in contact. Hence it was considered that an iron ship should exhibit in its general fabric the characteristic phenomena of one undivided mass, or a unity as a magnetic body.

These anticipations, it will be seen (published betwixt three and four years ago), have, so far as we have now time to elucidate them, had verifications, in actual experiments on iron ships, as beautiful as they are conclusive.

In the case of the 'Elba,' an iron ship of 200 feet in length, built recently on the Tyne, the magnetic condition before launching, which I was invited to investigate by Mr. Robert Newall, the owner, was found satisfactorily accordant with theory. Her head pointed south a few degrees westerly, and her lines of no-deviation (indicative of the position of the magnetic equatorial plane) were at a small distance in elevation different on the two sides, that of the starboard side being the highest. Proceeding in a direction at right angles to the dip, and passing through, or near to, the ship's general centre of gravity, the lines of no-attraction (descending forward) came out near the junction of the stem with the keel. And *there*, it was remarkable, as I had suggested as probable to Mr. James Napier of Glasgow, before making any experiment, there was found a departure from the ordinary regularity of the lines of no-attraction in a considerable downward bend.

Towards the stern, the equatorial lines rose out of and came above the iron plating of the *top-sides*, about 40 feet from the tafrail; thus giving to the *after-part* of the ship a uniform northern polarity. The ship, consequently, had become a huge simple magnet—the north pole at the stern and the south at the head. The attractive

power, as was expected, was highly energetic. At the distance of 50 feet, a compass on the level of the keel, at right angles to it abreast of the stern, was deviated to an extent of above 10°; at 100 feet distance the ship's magnetism caused a deflection of about half a point; and at 150 and even 200 feet there was a very sensible disturbance!

In the case of the 'Fiery Cross,' built at Glasgow and launched in January last (a case which I have elsewhere referred to), the lines of no-deviation, as taken for me by Mr. James Napier, were still more rigidly in accordance with theory,—the difference of elevation of the observed lines of no-deviation at the main-breadth section agreeing with calculation, theoretically, to within an inch or two. In the other case, that of the 'Elba,' a slight discrepancy as to the comparative level of the lines of no-attraction on the two sides, might, perhaps, be satisfactorily explained by the proximity and somewhat disturbing influence of another iron ship (advanced only to the frames or angle irons) on the port side of her.

In the case of the 'Elizabeth Harrison,' a large iron ship built at Liverpool, the first I had carefully examined, the correspondence of the magnetic polar axis and equatorial plane with those of terrestrial action was equally characteristic and conclusive.

Hence we may perceive a sufficient reason for some of the peculiar phenomena in iron ships of the compass disturbances and their changes. We may see why a ship built with her head easterly or westerly, and having the polar axis inclined over 18° or 20° to the starboard or port side, should be *particularly liable to compass changes*, if severely strained or struck by the sea with her head in an opposite direction. We see why the compasses of the 'Tayleur' should have been exposed to such a change as appears to have taken place in her lamentable case. We see in the case of the 'Ottawa*' (one I have elsewhere referred to), why a heavy blow of the sea, with the ship heeling and her head pointing eastward, would be likely to produce a change in her magnetism, when her previous magnetic distribution was solicited by terrestrial action in an angle of 30° or 40° of difference. And we see why the *deviations* of the compass in iron ships should, differently from those of wooden ships, be sometimes *westerly* and sometimes *easterly* in ships built and trading in home

* Where the compass suddenly changed two points.

latitudes; for here, whilst in wooden ships, where the iron work is in detached masses, the ship can have but little, externally, of the character of a true magnet, and can possess but small comparative differences from the position of her head whilst building; in iron ships, on the converse, where the ship is rendered by percussive action a powerful and, *retentively*, true magnet, her deviating action must be expected to be different, as the polarity of the head or stern may differ in denomination, or as the ship's magnetic polar axis may happen to lie over to starboard or port.

As an objection might be made to deductions from experiments on simple individual bars or plates of iron being applied to the case of iron ships built up of thousands of pieces, I have repeated the experiments, substituting for an entire plate or bar of iron a plate about 18 inches long and 3 broad, made up of numerous separate plates, and combined in the manner of the plating of iron ships. The compound or combined plate of some eighteen or twenty pieces yielded, under percussion, vibration or bending, results precisely similar to those obtained by the use of single plates or bars.

III. Extract of a Letter from Professor LANGBERG of Christiania to Colonel SABINE, dated June 10th, 1855. Communicated by Colonel SABINE, V.P. and Treas. R.S.

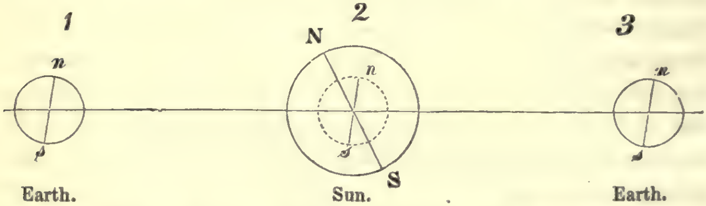
"Of all the important results from the discussions of the British Colonial Observatory, the discovery of the *direct* action of the sun on the magnetism of the earth is certainly a fact of the highest interest, in opening quite a new field for investigation; and few modern discoveries in this branch of science have interested me more than yours of the annual variation of the diurnal variation of declination. It seems that M. Secchi of Rome has nearly touched at the same discovery, and I am indeed glad that the enormous quantity of calculations, which you are superintending, did not prevent you from publishing your results before the ripening fruit was plucked by another. The first beautiful result of this annual variation is the explanation of the fact, which you have deduced from the observations at St. Helena and the Cape of Good Hope, that the horary variation

of declination does *not* vanish in passing from the northern to the southern magnetic hemisphere, but only changes signs at the equinoxes. I think every physicist will agree with you, that no thermic hypothesis will be able to explain this annual variation, but as you say (Toronto, ii. p. xix), it is 'obviously connected with, and dependent on, the earth's position in its orbit relatively to the sun, around which it revolves, as the diurnal variation is connected with the rotation of the earth on its axis.' But you have given no hint how this different position in its orbit can affect the magnetic condition of the earth, except so far as you suppose that the excentricity of the orbit is the reason that the total magnetic force is about $\frac{2}{1000}$ greater at the perihelion than at the aphelion (page xciii); but even granted that this variation is the effect of the excentricity, it cannot be the cause of the annual variation of the declination, as this is of contrary signs in the two semiannual periods.

"I have thought that this annual variation might possibly be explained by the following considerations, which I (although with extreme diffidence) shall venture to lay before you.

"As the recent magnetic observations have proved without doubt the *direct* magnetic action of the sun, or that the sun itself is a magnet, the sun must accordingly have magnetic polarity or magnetic poles. Now in our ignorance of the causes of the magnetic condition of the heavenly bodies, I think it reasonable to connect it in some way with their rotation on their axes, and to assume that generally their magnetic axis will nearly coincide with the axis of rotation; at all events, if these do not coincide, but include a small acute angle, the sum of the magnetic influences on a distant magnetic body during a whole rotation, will be nearly the same as if the magnetic poles were placed in the axis of rotation. If we suppose a magnet E revolving about another S, the magnetic axis of E remaining parallel with itself, but not parallel with the axis of S (as the earth around the sun), then the magnetic induction of S on E will depend on the relative position of both magnetic axes. Moreover, if we only regard the mean of the magnetic induction of the sun on the earth during several rotations of both on their axes, we may approximately assume that the magnetic axis of both coincides with the axis of rotation, and compare their relative position during a whole revolution or annual period, with the magnetic variation in

question. Now, if a plane is laid through the sun's axis parallel with the axis of the earth, this plane intersects the ecliptic in two points, whose longitudes are 286° and 106°, and has an inclination of 83°·2, both axes forming an angle of 25°·8. Accordingly, the northerly prolongation of both axes will *converge* (as in 1 and 2)



when the longitude is 286°, or about seventeen days after the summer solstice, and the southerly prolongation (as in 2 and 3) when the longitude of the earth is 106°, or sixteen days after the winter solstice. *About sixteen days after the equinoxes* the axes are in the position 2, and the radius vector forms the greatest angle with the above-named plane, viz. 83°·2. It is therefore evident, that the greatest magnetic induction takes place at the two solstices, but of opposite character, as the north poles converge in one, and the south poles in the other semiannual period: the change takes place *sixteen days after the equinoxes,* exactly as you have found by observation, and I regard this circumstance as very important evidence for my hypothesis, although you have shown that it could also be accounted for by the fact, that all magnetic induction takes some time ere it attains its maximum.

" If the sun's magnetic axis does not coincide with the axis of rotation, then I suppose we shall find, by more minute examination of the observations, that there exists also a small magnetic variation corresponding to the sun's geocentric rotation, or 27·68 days.

" There seems also to be strong reason to suppose that Hansteen's discovery about the annual periodic frequency of the aurora borealis, which has a marked maximum at the equinoxes, and even a more marked maximum at the solstices, is connected with the same cause."

IV. "Anatomical Notices." By Professor ANDREW RETZIUS, of Stockholm. Extracted from a Letter to Dr. SHARPEY, dated 10th May, 1855. Communicated by Dr. SHARPEY, Sec. R.S. (*Translation.*)

"1. *On the Form of the Cranium in the Embryo.*

"So far as I am aware, due attention has not hitherto been given to the different forms presented by the cranium in its earlier stages of growth. In the skeletons of early human embryos to be seen in most museums, the imperfectly ossified cranium is for the most part shrunk up and disfigured. To obtain a correct view of the form of the cranial cavity, I first remove the skin, fascia and muscles; I then, by injecting water through the vertebral canal, thoroughly wash out the soft brain and spinal cord; and lastly, fill the cerebro-spinal cavity with quicksilver or with melted tallow, taking care not to distend it over-much. The opening in the vertebral canal is to be stopped with a little plug of wood, and the preparation allowed to dry.

"In the skeleton of a human embryo of the fourth month, prepared in this way, the occipital bone was found to have the form of a funnel, the narrow part of which passed into the vertebral canal, as represented in the accompanying figure 1.

"It thus appears that the human occipital bone, in its early condition, approaches in form to the vertebral canal, and in this respect also it resembles the occipital bone in several quadrupeds, which so obviously represents the first cephalic vertebra.

"For the sake of comparison, I divided the skull of another embryo of the same age into two halves by a vertical median section, washed out the brain and examined the preparation while it lay immersed in weak spirit of wine in a shallow glass capsule. The occipital bone had the same funnel-shape as in the former case. As development advances, the funnel-like form is gradually lost, whilst, on the other hand, the bone appears more deepened or tubular the earlier the embryo to which it belongs. The same is true of quadrupeds.

"In the beautiful figure of the embryo-skull, given in Kölliker's 'Microscopische Anatomie,' B. II. taf. 3. fig. 2, the downward pro-

longation of the occipital bone appears much less, but whether less than it ought to be I cannot venture to say, as I happen to have no specimen of that age (one month older than the one I have given) with which to compare the figure.

Fig. 1.

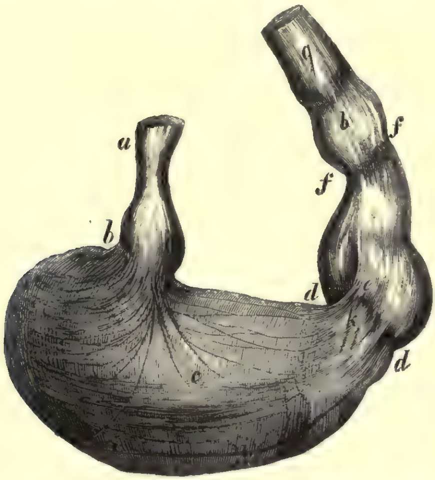
" The early form of the occipital bone I have here described becomes easily intelligible when we remember that the shape of the skull is regulated by that of the brain; and that as the latter is at first greatly elongated, and its ganglia imperfectly covered, the cranium must then also have a correspondingly elongated form.

" 2. *On the Antrum Pylori.*

" I have now for some time directed my attention to the determination of the true form of the stomach, and have become more and more convinced that the antrum pylori of the older anatomists ('Pförtnerhöhle' of the Germans) is really a special compartment of the general cavity. I have had occasion to make numerous examinations of the stomach in the bodies of middle aged women who died in the hospital, and found the form to be nearly as represented in figure 2, where *d d*, *f f* indicate the antrum pylori. This part is also distinguished by greater thickness of its muscular coat, more strongly

developed glands, and the presence of the well-known plicæ fimbriatæ. The commencement of the duodenum also forms a special rounded cavity, which I should propose to name the Antrum Duodeni, and which is characterized internally by the absence of valvulæ conni-

Fig. 2.



ventes, and by the dense array of Brunner's glands beneath its mucous membrane. This part constitutes what has been called the fourth stomach of the Porpoise, and some other Cetaceans. I may observe that the so-called ligaments of the pylorus are connected with the formation of the antrum pylori.

"3. *On the Fascia Superficialis.*

"The accounts usually given of the fascia superficialis are for the most part very imperfect. As far as I can judge, this fascia is in many parts of the body a constant membrane, and really appertains to the skin, as may be particularly well seen in the integuments of the back. The cutaneous muscle of quadrupeds, in most cases, probably arises out of this fascia; the muscular fibres being deposited, as I conceive, in the midst of its substance, and finally becoming covered by it as their perimysium. In this way too, I imagine the platysma myoides and epicranius to be formed."

V. "On the Effect of Local Attraction upon the Plumb-line at stations on the English Arc of the Meridian, between Dunnose and Burleigh Moor; and a Method of computing its Amount." By the Venerable Archdeacon PRATT. Communicated by the Rev. J. CHALLIS, F.R.S. Received June 5, 1855.

The author states that in a former communication he had pointed out a method for calculating the deflection of the plumb-line at stations on the Indian arc, caused by the attraction of the Himalayas and of the vast regions beyond, with a view to the correction of the astronomical amplitudes of the measured subdivisions of the arc before they are applied to the determination of the ellipticity of the earth.

The same subject is taken up in the present paper, but in reference to the English arc between Dunnose and Burleigh Moor; and a different method of calculating the attraction is given.

The paper consists of three parts. In the first, the ellipticity of the English arc is calculated without taking account of attraction. The arc is divided into five parts, and the lengths and amplitudes assigned to them in Mudge's Trigonometrical Survey of England, vols. ii. and iii., are made the basis of the calculation. These portions of the arc are compared two and two, and ten values of the ellipticity thence deduced; the mean of which is $-\frac{1}{43\cdot8687}$. The ten values, of which this is the mean, differ considerably among each other, indicating that there is some disturbing cause, like local attraction, affecting the plumb-line, and therefore the apparent latitudes. The variations of the observed amplitudes are then discussed; and the necessity of calculating the local attraction pointed out.

In the second part a formula is obtained for calculating the attraction. The method is different from that given by the author in his first communication. The curvature of the earth is neglected, as this would have no sensible effect on the results in the British Isles. The attracting mass is divided into a number of smaller masses standing on rectangular bases at the sea-level, and the height of each

is taken equal to the average height of the surface above the sea-level. The dimensions of the bases may differ from each other, and are determined by the contour of the surface in such a way that the average height in each mass may not depart materially from the height of any part of it. The investigation leads to the following Rule for determining the horizontal attraction deflection of the plumb-line caused by any one of these Tabular Masses (as the author calls them) :—

Rule.—*Take the origin of coordinates at the station where the plumb-line is. Let the plane of* xy *be horizontal, and the axis of* x *in the vertical plane in which the amount of deflection is to be found.*

Write down the coordinates XY xy *of the furthest and nearest angles of the Tabular Mass from the origin;* Y *is always to be considered* $+^{\text{ve}}$, *and* y $+^{\text{ve}}$ *or* $-^{\text{ve}}$ *accordingly.*

Form four ratios, by first dividing each ordinate by the abscissa not belonging to it, and then by dividing each ordinate by its own abscissa, viz.

$$\frac{Y}{x}, \ \frac{y}{X}, \ \frac{Y}{X}, \ \frac{y}{x}.$$

Look in a Table of Tangents for the four angles of which the tangents equal the above ratios.

Form four more angles by adding (subtracting if they be negative) half of each of these angles just found to (or from) 45°.

From the sum of the log-tangents of the first two of these angles subtract the sum of the log-tangents of the second two.

This result, multiplied by H *feet and by* $\frac{1''}{369}$, *will give the required deflection of the plumb-line in seconds of a degree*—H being the height of the Tabular Mass above the sea-level, and its density being taken equal to half the mean density of the earth, which is that of granite.

The only restriction to be attended to in the application of this Rule is, that the ratio of the height of the attracted station above the sea to each of the horizontal coordinates of the nearest angle of the Tabular Mass, must be so small that its square may be neglected.

If any part of the attracting mass is nearer to the station than this, it must be divided into vertical prisms and the attraction of each found; for which the author gives a formula in a note.

In the third part the Rule is applied, for the purpose of illustration, to obtain an approximate value of the deflection of the plumb-line at Burleigh Moor, the north station of the arc under consideration, situated on the north coast of Yorkshire. The deflection is found to be $3''\cdot644$ to the south. The data upon which this calculation is based are gathered by the author from the Map which accompanies General Mudge's account of the English Survey, and the heights marked down on that map.

The deflection at several other stations is deduced from this result of calculation, by using the amplitudes given in Mudge's work, and also in Captain Yolland's 'Astronomical Observations made with Airy's Zenith Sector,' published in 1852, and by supposing the curvature of the meridian to be the mean curvature of the whole globe as laid down by Mr. Airy in his article on the Figure of the Earth. Thus the deflection at Black Down on the Dorsetshire coast (one of the places mentioned in Captain Yolland's volume), the author finds to be $5''\cdot886$ to the north, a quantity which tallies well with the deflection assigned to Burleigh Moor on the Yorkshire coast, if the relative heights of the two coasts are compared. This affords a satisfactory evidence of the correctness of the principles laid down in the paper; and, as the author thinks, makes the subject well worthy the attention of those who are interested in the English Survey, and who have it in their power to furnish the most accurate data for the application of the Rule he lays down. The subject is also no less important to the mathematician in his investigation of the figure of the earth.

In a Postscript the author makes the following remarks upon the Astronomer Royal's method of accounting for the large amount of deflection on the Indian arc deduced by the author in his former communication :—

" Since the above was written, I have had the opportunity of seeing a notice of the communication of the Astronomer Royal on the Density of Table-lands supposed to be supported by a dense fluid or semi-fluid mass; and the use he makes of his suggestions to remove the discrepancy, pointed out in my first communication, between the values of the deflection of the plumb-line in India, as determined by calculating the attraction of the Himalayas, and as indicated by the results of the Great Trigonometrical Survey. The following diffi-

culties occur to me in the way of this highly ingenious and philosophical method of removing the discrepancy :—

"1. It assumes that the hard crust of the earth is sensibly lighter than the fluid or semi-fluid mass, imagined to be a few miles below the surface. But I know of no law, except the unique law of water and ice, which would lead us to suppose that the fluid mass in consolidating would expand and become lighter. One would rather expect it to become denser, by loss of heat and mutual approximation of its particles.

"2. There is, moreover, every reason to suppose, that the crust of the earth has long been so thick, that the position of its parts relatively to a mean level cannot be any longer subject to the laws of floatation. If the elevations and depressions of the earth's surface have always remained exactly what they were at the time when the laws of floatation ceased to have an uncontrolled effect, then the same reasoning would no doubt apply in our case, as if they still had their full sway. But geology shows that other laws are in constant operation (arising most probably, as Mr. Babbage has suggested, from the expansion and contraction of the solid materials of the crust), which change the relative levels of the various parts of the earth's surface, quite irrespectively of the laws of floatation. If Mr. Hopkins's estimate of the thickness of the crust be correct, viz. at least 1000 miles, these laws of change in the surface must have been in operation for such an enormous interval of time, as quite to obliterate any traces of the form of surface which the simple principles of hydrostatics would occasion. Indeed, it seems to me highly probable that the elevation of the Himalayas and the vast regions beyond may have arisen altogether from the slow upheaving force arising from this cause.

"I am inclined to think that the only explanation of the discrepancy between my calculations and the results of the Indian Survey, is to be found in the greater curvature of the Indian Arc."

VI. "Contributions to the History of Aniline, Azobenzole and Benzidine." By A. W. Hofmann, Ph.D., F.R.S. Received May 30, 1855.

The transformation of nitrobenzole into aniline by the action of sulphuretted hydrogen is attended with such difficulties and requires especially so much time, that chemists hitherto have generally preferred to prepare this base from indigo. Lately a new modification of Zinin's process has been adopted by M. A. Béchamp*, which consists in submitting nitrobenzole to the reducing action of acetate of protoxide of iron. This process—M. Béchamp simply uses a mixture of iron and acetic acid—is applicable to all nitro-compounds, and has been extensively tried with the most perfect success in the laboratory of the Royal College of Chemistry. The facility of the process, its rapidity, and the large amount of product it yields in most cases, cannot fail materially to assist the study of the volatile organic bases.

During these experiments several observations were made, which I beg leave to bring under the notice of the Society.

When employing about double the amount of iron which is recommended by M. Béchamp (2·5 instead of 1·2 of iron to 1 part of nitrobenzole), Mr. Alfred Noble found that the latter portion of the distillate solidified partly in the receiver and partly in the condenser. Washed with hydrochloric acid from adhering aniline, and once or twice recrystallized from boiling alcohol, the solid matter was obtained in fine crystals of a yellowish-red colour, and fusing below the boiling-point of water. These crystals possessed all the properties of azobenzole, which was moreover identified by combustion.

0·260 gramme of substance gave 0·755 grm. of carbonic acid and 0·134 grm. of water.

The well-established formula of azobenzole, $C_{12}H_5N$, requires the following values:—

	Theory.		
	Equivalents.	Per-centage.	Experiment.
Carbon	12	79·12	79·12
Hydrogen	5	5·49	5·76
Nitrogen......	1	15·39	
		100·00	

* Annales de Chimie et de Physique, (3) tom. xlii. 186.

The azobenzole obtained in this manner is so readily purified that this process is greatly preferable to the action of alcoholic potassa upon nitrobenzole, since the latter process simultaneously produces several substances which can be separated only with difficulty from the azobenzole.

A portion of the azobenzole obtained in the above process was converted by means of sulphuretted hydrogen into benzidine. Of the beautifully crystallized compound, a platinum salt was made which was analysed by Mr. Noble.

0·268 grm. of the salt left 0·088 grm. =32·88 per cent. of platinum. The formula $C_{12}\ H_6\ N,\ HCl;\ Pt\ Cl_2$ requires 33·09 per cent. of platinum.

Benzidine exhibits a very interesting deportment with nitrous acid; gently warmed in the gas of this acid, obtained by treatment of starch with nitric acid, it gives rise to a powerful reaction. The substance assumes an orange-red colour, and exhibits, after treatment with water, when recrystallized from alcohol, all the properties of azobenzole.

The reproduction of this body from benzidine was moreover fixed in the following numbers:—

0·215 grm. of substance gave 0·623 grm. of carbonic acid, and 0·112 grm. of water.

	Theory. ($C_{12}H_5N$).	Experiment.
Carbon	79·12	79·02
Hydrogen	5·49	5·78
Nitrogen	15·39	
	100·00	

The simplest formulæ of azobenzole and benzidine only differ by one equivalent of hydrogen,—

Azobenzole...... $C_{12}\ H_5\ N$

Benzidine ...... $C_{12}\ H_6 N$,

a relation which sufficiently explains the transformation and reproduction of azobenzole.

VII. "On the Formation and some of the Properties of Cymidine, the Organic Base of the Cymol Series." By the Rev. JOHN BARLOW, F.R.S., Sec. R. Inst. Received June 14, 1855.

The object of this memoir is to detail the process by which an organic base, provisionally named *Cymidine*, was obtained from the hydrocarbon, cymol, and to describe some of its properties, and certain phenomena attending its production.

The substitution-product, nitrocymol, was procured by acting on cymol by strong nitric acid, both liquids being kept at the temperature $-17\frac{7}{9}°$ Cent. (0° Fahr.). From nitrocymol, cymidine was obtained by Béchamp's modification of Zinin's process, and results of analyses, made by combustion of the platinum salt, and likewise by a silver determination of the hydrochlorate, were found to coincide with the formula $C_{20} H_{15} N$. In the formation of cymidine a neutral oil occurred, having the same boiling-point with cymol. From this hydrocarbon a substitution compound was derived, apparently isomeric with, but possessing a less specific gravity than nitrocymol. This nitro-compound was also subjected to the process of reduction already described, and a basic substance was formed from it, which was identified by a platinum determination with cymidine. Some qualitative experiments, made with cymidine, were also described, and the memoir concluded with the following synoptical table of the homologues of the benzol series.

Hydrocarbons.	Nitro-substances.	Bases.
Benzol $C_{12} H_6$	Nitrobenzol $C_{12} H_5 NO_4$	Aniline $C_{12} H_7 N$
Toluol $C_{14} H_8$	Nitrotoluol $C_{14} H_7 NO_4$	Toluidine $C_{14} H_9 N$
Xylol $C_{16} H_{10}$	Nitroxylol $C_{16} H_9 NO_4$	Xylidine $C_{16} H_{11} N$
Cumol $C_{18} H_{12}$	Nitrocumol $C_{18} H_{11} NO_4$	Cumidine $C_{18} H_{13} N$
Cymol $C_{20} H_{14}$	Nitrocymol $C_{20} H_{13} NO_4$	Cymidine $C_{20} H_{15} N$.

VIII. Letter from Dr. HERAPATH to Professor STOKES, "On the Compounds of Iodine and Strychnine." Communicated by Professor STOKES, Sec. R.S. Received June 21, 1855.

June 20, 1855.

MY DEAR SIR,—Will you do me the favour to announce to the Royal Society, that I have been engaged, during some months past, in investigating the optical and chemical properties of some crystalline compounds of iodine and strychnine which appear to be strongly indicative and peculiar? One of these bodies, from the analysis hitherto made, would seem to have a formula not very different from the following, viz. $C^{42} H^{22} N^{2} O^{4} + I^{3}$, and crystallizes in hexagonal prisms, passing by the ordinary replacement planes to the acute rhombohedron and other forms, all apparently derived from the rhombohedric system; some of these crystalline forms are very strange and unusual. This substance possesses "double absorption" in a very evident degree, and when examined by vertically plane polarized light, the hexahedral prisms are all obstructive of the polarized beam when the length of the prisms lies parallel to the plane of the incident ray; in this position they appear dark sienna-brown in colour; when the long axis of the prisms lies perpendicular to the plane of primitive polarization the crystals transmit a lemon-yellow tint, passing through greenish yellow to sherry-brown.

The other substance appears to be the sulphate of iodo-strychnia, and has a decidedly metallic green reflexion, crystallizes in stellate aggregations of prisms, brilliantly green by reflected light, but having a deep blood-colour by transmission; these also possess double absorption and are very peculiar, as a slight increase in thickness renders them wholly impervious to light.

When these matters have been more carefully worked out, I hope to have the pleasure of communicating the results to the Society: in the mean time the present notice will be sufficient for the object in view.

I remain, my dear Sir,

Yours very truly,

W. BIRD HERAPATH.

Professor Stokes, F.R.S.

IX. "On the Existence of a Magnetic Medium." By T. A. Hirst, Esq. Communicated by Dr. Tyndall, F.R.S. Received April 14, 1855.

In a note on the above subject, communicated to the Royal Society on March 16, 1855, Professor Williamson objects to a certain conclusion deduced by Professor Tyndall in a letter addressed by the latter to Mr. Faraday and recently published in the Philosophical Magazine. Professor Tyndall's conclusion was that, according to the hypothesis of the existence of a magnetic medium in space and of the identity of magnetism and diamagnetism, a compressed diamagnetic cube ought to be less repelled when the magnetic force acts on the line of compression than when it acts at right angles to that line; a result which his own experiments have contradicted. Against the legitimacy of this conclusion Professor Williamson urges that "Dr. Tyndall seems to have assumed, that on the compression of an aggregate of particles of a diamagnetic substance, the medium is not displaced by the particles in their change of position." We shall be better able to estimate the value of this objection by recalling the steps of Professor Tyndall's argument.

A magnetic cube was taken which had *already* been compressed: its deportment before a magnet was experimentally examined, and deductions drawn concerning the changes that would occur in that deportment by merely conceiving the magnetic capacity of the material particles to be diminished, without in any way altering the distances between those particles, and consequently without in any way displacing the magnetic medium in the interstices of the body.

Instead of the assumption attributed to Dr. Tyndall, he might, with greater justice, be accused of having disregarded the possible presence of the medium within the body; but in his own defence he may with perfect justice reply, that in Mr. Faraday's experiments, which originally gave rise to the discussion, no such interpenetration of two media existed.

Admitting, however, that the interstices of a body are occupied by the medium, it may be interesting to inquire whether, from an argument similar to Professor Tyndall's, the same decided conclusion could, with equal accuracy, be deduced. To answer the in-

quiry, it must be remembered that the force of the argument in question depends essentially upon the justness of the supposition, that a diamagnetic cube may, theoretically, be produced from a magnetic one by conceiving the magnetic capacity of the particles of the latter to be sufficiently diminished. It is evident that the total attraction of the cube by a magnet will be equal to the sum of the attractions of the material particles, and of the medium contained in its interstices. If this sum be greater than the attracting force upon the quantity of medium which the cube *and its contents* displace, the substance is called magnetic, for it will be drawn towards the magnet; if less, it is called diamagnetic, for it will be repelled from the magnet. But in our present knowledge of the properties of the medium there is nothing incompatible with the supposition that the density of the internal medium may so far exceed that of the external, that the attraction of the former by the magnet is *itself* greater than the attraction of the medium displaced by the cube and its contents. If so, however, no conceivable diminution of the magnetic capacity of the material particles could possibly render such a cube diamagnetic.

This is sufficient to show that, admitting the presence of the medium within the cube, the method of argument adopted by Professor Tyndall would not be strictly applicable, unless the density of the internal medium were subjected to limits which the advocates of its existence might possibly be unwilling to grant.

But it may be asked, if, whilst admitting that the medium may exist in the interstices of the body, it be granted that a diamagnetic may be produced from a magnetic cube in the manner assumed by Professor Tyndall, does it still follow, necesarily, that attraction is always greatest—repulsion least—when the force acts in the line of compression? In other words, can a conclusion contradictory to experimental facts be *then* legitimately deduced?

In attempting a reply to this question, it will, perhaps, be best to employ the following symbols. Let W represent the attracting force of the magnet upon the medium displaced by the cube and its contents. The value of W will, of course, be unaltered, no matter whether the force acts in, or at right angles to the line of compression. When the force acts in the line of compression, let P_1 represent the attracting force upon the particles, W_1 the attracting force

upon the internal medium, and let F_1 be proportional to the resultant attraction of the cubical mass towards the magnet. Let P_2, W_2 and F_2 have similar significations when the force acts at right angles to the line of compression. Then we may put

$$F_1 = P_1 + M_1 - W,$$
$$F_2 = P_2 + M_2 - W.$$

Now, in a compressed magnetic cube, experiment proves that

$$F_1 > F_2,$$

or $$P_1 + M_1 > P_2 + M_2,$$

i. e. $$P_1 - P_2 > -(M_1 - M_2).$$

As long as we are ignorant of the properties of the medium within the body, we will, for the sake of completeness, consider the following three distinct cases.

I. The attracting force upon the medium within the cube is the same when the force acts in either the one or the other of the two directions, with respect to the line of compression. Here

$$M_1 = M_2,$$

hence $$P_1 > P_2.$$

II. In whichever of the two directions of the force the attraction of the particles may be greatest, the attraction of the internal medium is also greatest in the same direction. Here, according as P_1 is greater or less than P_2, M_1 is greater or less than M_2; hence, inasmuch as

$$P_1 + M_1 > P_2 + M_2,$$
$$P_1 > P_2 \text{ and } M_1 > M_2.$$

III. In whichever of the two directions of the force the attraction of the particles may be greatest, the attraction of the internal medium is greatest in the direction perpendicular to the same. Here, according as P_1 is greater or less than P_2, M_1 is less or greater than M_2, so that the two hypotheses

(1) $P_1 > P_2$ and $M_1 < M_2$, and
(2) $P_1 < P_2$ and $M_1 > M_2$

are both compatible with the sole condition,

$$P_1 + M_1 > P_2 + M_2.$$

In order to test the applicability of Professor Tyndall's method of argument in each of these cases, let us conceive, with him, that the magnetic capacity of the particles is so far diminished that their attractions P_1 and P_2 are reduced to p_1 and p_2. This may evidently be done by making $p_1 = aP_1$ and $p_2 = aP_2$, where a represents a positive fraction whose magnitude can be diminished indefinitely. If, under this supposition, the resultant attractions F_1 and F_2 become f_1 and f_2, then it can easily be proved that, however small the value of a may be, we shall always have

$$f_1 > f_2$$

in the cases I., II. and III. (1). That is to say, with the distributions of the internal medium assumed in these cases, the resultant attraction of the cubical mass will always be greatest, or repulsion least, when the force acts in the line of compression, no matter how diamagnetic the cube may have become by diminishing the magnetic capacity of its particles. It may just as easily be proved, however, that in case III. (2) a value of a may be chosen sufficiently small to make

$$f_1 < f_2,$$

that is to say, with the distribution and properties of the internal medium here supposed, it is quite possible so far to diminish the magnetic capacity of the particles as to obtain a cube which will be attracted least, or repelled most strongly when the force acts in the line of compression. This conclusion involves nothing contradictory to experimental facts, whereas the former one does.

I will not here enter into the question of the relative probability of these three cases, supposing the medium to exist. My sole object has been to show that, although the method of argument adopted by Professor Tyndall is strictly applicable in a great number of cases, even when the medium is supposed to fill the interstices of the body, yet it is possible to attribute properties to this medium of such a nature as to avoid the conclusion, contradictory to fact, which he has deduced. This may be done in two ways. *First*, the density of the internal medium may be such as to render it impossible to produce a diamagnetic from a magnetic cube in the manner assumed, *i. e.* by diminishing the magnetic capacity of the material particles. *Secondly*, granting that a diamagnetic cube may be so

produced, the distribution and properties of the internal medium may still be such as to cause the cube to be attracted least, or repelled most strongly when the force acts on the line of compression, and thus, if the substance be diamagnetic, to cause it to agree, in its deportment, with experimental results. On the other hand, if these hypothetical properties of the internal medium be discarded as artificial or inadmissible, then at present I see no way of escaping the conclusion of Professor Tyndall's argument.

With regard to the explanation given by Professor Williamson, it will be observed that he pursues a path quite different from that of Professor Tyndall, when he considers the effects produced by compressing a number of particles surrounded by a magnetic medium. This compression, he states, may alter the attraction of the mass by a magnet in two ways;—"first, by altering the density of the matter; secondly, by altering the density of the medium." By the term '*density of matter*' is usually understood the relation which exists between the quantity of matter which a body contains, and the volume of the space enclosed by its external surface. But in the present case, where a comparison is instituted between the matter of the body and the medium which is supposed to fill all its pores, we must, I imagine, understand by the term 'density of matter,' the relation which exists between the sum of the masses of the particles, and the sum of their volumes; but if so, then, the particles themselves being incompressible, it is clear that compression could not alter the 'density of matter.'

As to the second effect of compression, viz. an alteration of the density of the medium, it may be quite conceivable, although I do not find that Professor Williamson has any where taken it into consideration. The effects of compression may, therefore, be more correctly described as either—*first*, a diminution of the interstices of a body, without altering the density of the medium which fills them; or *secondly*, a diminution of the interstices, accompanied by an alteration of the density of the medium within them.

Let us assume, as Professor Williamson has virtually done, that the first of these effects takes place; then, if we admit that "in a cubical mass of carbonate of iron the material particles are more magnetic than the medium which they displace, and the force with which it is attracted is proportional to this excess," we can by no

means admit that, because "it becomes more magnetic by compression, we must conclude that the loss of magnetic medium *from its interstices* is more than supplied by the magnetic matter which takes its place;" for, according to what has already been advanced, the excess of the attraction of the material particles above that of the medium they displace is the same after, as it was before compression; inasmuch as compression merely changes the relative *situations* of the particles, by bringing them closer together, but does not in the least alter their volume, and consequently does not in the least alter the quantity of medium they displace.

With respect to carbonate of lime, Professor Williamson's conclusion is, of course, untenable, because it is based upon the foregoing one. He says, "when these particles are brought closer together by pressure, with diminution of the intervening spaces occupied by the medium, the mass becomes more diamagnetic, because a certain quantity of the magnetic medium is thus replaced by the less magnetic matter." It is, however, manifest that exactly the same quantity of magnetic medium is displaced by the less magnetic matter after, as there was before compression. Why, then, should diamagnetic action be increased by compression?

Lastly, with respect to the crystals of carbonate of iron and carbonate of lime, Professor Williamson's explanation, although ingenious, is liable to the same objections as those already mentioned. It cannot, in fact, be said that the functions of matter predominate most strongly over those of the medium they displace in any one direction, merely because the particles may be closer together in that direction; for, as long as each particle is surrounded by the medium, the predominance of that function of the particles with which we are concerned, *i. e.* their attraction, over that of the medium they displace will be the same, whatever may be the distances of the particles asunder.

From the foregoing remarks, therefore, it is manifest that, if Professor Tyndall has not yet succeeded in demonstrating that the hypothesis of the existence of a magnetic medium and of the identity of magnetism and diamagnetism is *necessarily* at variance with experimental facts, neither has Professor Williamson succeeded in proving that this hypothesis is in accordance with those facts. The question of the existence of a magnetic medium is still an open one,

and will continue to be so until the many important principles which it involves, but which have not been introduced into the present discussion, have been further elucidated by new investigations and new thoughts.

X. "On the ultimate arrangement of the Biliary Ducts, and on some other points in the Anatomy of the Liver of Vertebrate Animals."* By LIONEL S. BEALE, M.B., Professor of Physiology and Morbid Anatomy in King's College, London. Communicated by F. KIERNAN, Esq., F.R.S. Received June 14, 1855.

In his valuable communication to the Royal Society in 1833, Mr. Kiernan describes and figures anastomoses between branches of the biliary ducts in the left triangular ligament of the human liver. The same author considered that the interlobular ducts anastomosed with each other, and communicated with a lobular biliary plexus, although he had never succeeded in injecting this plexus to the extent shown in his figure, neither had he directly observed the anastomoses between interlobular ducts. It must be borne in mind that these observations were made before the liver-cells had been described.

Since the appearance of Mr. Kiernan's paper, various hypothetical views have been advanced by different observers, with reference to the arrangement of the minute biliary ducts and the relation which the liver-cells bear to them. These points, however, have not yet been decided by actual observation.

Müller considered that the ducts terminated in blind extremities. Weber showed that the right and left hepatic ducts anastomosed by the intervention of branches in the transverse fissure of the liver, which he described under the name of Vasa aberrantia.

Krukenberg, Schröder Van der Kolk, Retzius, Theile, Backer, Leidy and others have adopted the view that the liver-cells lie within a network of basement membrane. On the other hand, Handfield Jones and Kölliker describe the liver-cells as forming a solid net-

work, against the marginal cells of which Kölliker believes the extremities of the ducts impinge, while Handfield Jones holds that the ducts terminate by blind extremities.

Henle, Gerlach, Hyrtl and Natalis Guillot look upon the finest gall ducts as communicating with intercellular passages.

Dr. Handfield Jones looks upon the small cells in the extremities of the ducts as the chief agents in the formation of bile, and to the liver-cells he assigns an office totally distinct from this. Busk and Huxley concur in this view, which would place the liver in the category of vascular glands, spleen, suprarenal capsules, &c.

The observations of the author have been made upon the livers of several different animals examined under various circumstances. The results of the examination of injected preparations precisely accord with the observations made upon uninjected specimens some months before. The points which he hopes to establish are as follows:—

1. That the hepatic cells lie within an exceedingly delicate tubular network of basement membrane.

2. That the smallest biliary ducts are directly continuous with this network.

3. That at the point where the excretory duct joins the tubes which contain the secreting cells, it is very much constricted, being many times narrower than the tube into which it becomes dilated.

Lobules.—With reference to the nature of the lobules of the liver, the author offers some remarks. The only liver in which he has been able to detect distinct lobules, consisting of perfectly circumscribed portions of hepatic structure and separated from each other by fibrous tissue, is that of the pig. In this liver each lobule has a distinct fibrous capsule of its own, and is separated from its neighbours by the branches of the vessels and duct for their supply.

The lobules of the liver of other animals are not thus separated from each other, but the capillary network and the cell-containing network of one lobule are respectively connected with those of the adjacent lobules at certain points between the fissures in which the vessels and duct lie. In these livers there is not a trace of fibrous tissue between the lobules.

The exceptional liver of the pig, with its distinct lobules, seems to bear in structural peculiarity the same relation to the livers of other animals, as the much-divided kidney of the porpoise bears to the more solid organ of most mammalian animals.

In a *physiological* sense the livers of all vertebrate animals may be said to be composed of lobules; but in a strictly *anatomical* sense this term can only be used with reference to the liver of the pig, and, according to Müller, that of the polar bear. The vessels and duct, at their entrance into the liver, are invested with much areolar tissue, which is continued for a considerable distance along the portal canals; but it gradually ceases as the vessels become smaller, and, with the exception of the liver of the pig above referred to, the lobules are not separated from each other by any areolar tissue, or by any fibrous tissue whatever, neither is any prolonged into their substance. Hence the investment of areolar tissue round the vessels in the portal canals of the liver seems to present no peculiar characters in its distribution. It must be borne in mind, that in the examination of uninjected specimens the small vessels and ducts are liable to be much stretched and torn in manipulation, and, in consequence, a striated appearance is produced which closely resembles fibrous tissue.

Method of preparing specimens.—In order to demonstrate the arrangement of the ducts described by the author, it is absolutely necessary to harden the liver previously. This hardening may be effected by soaking a portion of the liver for some time in strong syrup, or in alcohol, and afterwards rendering the section transparent by soda. The mixture of alcohol and acetic acid recommended by Mr. L. Clarke in his investigations upon the spinal cord, has also been employed, as well as many other solutions which are not described. The fluid to which the author gives the preference is alcohol, to which a few drops of solution of soda have been added.

Method of injecting the biliary ducts.—The following is the method by which, after numerous trials, the author succeeded in effecting this object. Lukewarm water is injected into the portal vein. After a time, when the liver has become fully distended, much bloody water will escape from the hepatic vein, but at the same time it will be remarked that bile escapes from the duct. This

bile gradually becomes thinner, and at last nearly pure water flows from the duct, showing that the bile has been washed out. The liver is now placed in soft cloths to soak up the water, and after some hours it will be found to have diminished much in volume, and to have a clayey consistence. The ducts are now empty, and may be injected with a carefully prepared prussian-blue injection, to which a little alcohol has been previously added. The mixture is to be well stirred, and after having been carefully strained, it is slowly and cautiously injected into the duct. Plain clear size is next thrown into the portal vein, until the liver has become fully distended with it in every part. Lastly, a little plain size is injected into the duct, the vessels carefully tied, and the liver placed in cold water until the size has set, when very thin sections can be readily obtained with the aid of a sharp knife. The author has tried many other plans of injection, but the above has afforded the most satisfactory results. On one occasion a human liver was successfully injected with four different colours; the portal vein with flake-white, the artery with vermilion, the duct with prussian blue, and the hepatic vein with lake.

Evidence of the existence of a tubular basement membrane in which the liver-cells are contained.

Not unfrequently liver-cells are set free with shreds of delicate membrane attached to them, and this can sometimes be seen to be prolonged either way in the form of a narrow tube.

In certain specimens which have been exposed for some time to the action of dilute soda, the walls of the cells appear to be dissolved and the tubes are seen to be occupied with a highly refractive mass, and their outline is rendered very distinct.

When portions of the cell-containing network are placed in strong syrup or glycerine, exosmose of the water occurs, the diameter of the tubes is much diminished, and their outline becomes distinct, but uneven, in consequence of the shrunken state of the tubes of the network.

At the edge of a very thin section of liver stretching between two capillary vessels, a very thin membrane, recognizable only by the granular matter adhering to it, can sometimes be seen.

The tubes of the network can be distended to a great extent by

injection, so that the walls of contiguous tubes meet, while the capillary vessel between them becomes so compressed as not to be recognizable.

The injection often forms a sharp line towards the capillary vessels on either side of the tube in which the cells lie, and gradually shades off towards the centre of the tube.

The cells which escape into the surrounding fluid from injected specimens often have portions of injection adhering to them.

If a section be made at right angles to the intralobular vein, the cells are seen to form lines radiating from the centre towards the circumference of the lobule, as authors have before described. These lines of cells are really tubes of basement membrane, communicating with each other at intervals by narrow branches. In injected specimens the walls of the tube can be demonstrated, and are seen to be distinct from the capillary vessels.

In the fœtus, the cells are seen to be separated from the cavity of the vessels by two lines separated by a clear space. One of these lines is caused by the outline of the tube containing the cells, the other is that of the capillary wall.

The author supposes that, originally, the liver is composed of a double network of tubes (cell-containing network and capillary network), the walls of which in most situations become incorporated, so that the secreting cells are only separated from the blood by one thin layer of basement membrane, which is very permeable to water in both directions, but the greatest force which can be applied without causing rupture is incapable of forcing bile through it.

Of the contents of the tubular network of basement membrane, and of the arrangement of the cells within it.

Within the tubular network lie the hepatic cells, with a certain quantity of granular matter and cell débris, and, in some instances, free oil-globules and granules of colouring matter. The cells are not arranged with any order or regularity. Some observers have endeavoured to show that the hepatic cells are arranged in a definite manner. Professor Lereboullet, one of the latest writers on this subject (1853), describes the cells as forming double rows. The two rows of cells may be separated by injection, and he gives two diagrams to illustrate their arrangement. The author has never

seen anything like this in any liver which has been examined by him. In Mammalia, according to his observation, the cells are for the most part arranged in single rows (human subject, pig, dog, cat, rabbit, horse, seal, Guinea-pig and others), but in some situations two cells lie transversely across the tube, and they may be forced into this position by injection. The cells do not completely fill the tubes, and are not always placed quite close together, being surrounded with granular matter. Injection passes sometimes on one side of the tube, and sometimes upon the other; often it entirely surrounds a cell. In the human fœtus and in the fœtal calf there are two or three rows of cells within the tubes, and this is also the case in the livers of most adult reptiles and fishes which have fallen under the author's observation, and in many parts of the network of the bird's liver.

Of the Ducts of the Liver.

The duct, like the artery, lies close to the portal vein; usually this vessel is accompanied by one branch of the artery and duct, but not unfrequently there are two or three branches of these vessels with the vein.

Anastomosis of the ducts near the trunk from which they come off.—The author observes that the anastomoses between the larger ducts and between the larger branches of the interlobular ducts are pretty numerous in the human liver, but these communications take place only near the origin of the trunks by means of intermediate branches. Different interlobular ducts do not anastomose with each other, but the branches resulting from the division of a small trunk are often connected together.

In some animals these communications are so numerous, that a complete network is formed at the portal aspect of the lobule, or around a small branch of the portal vein.

Not only are the right and left hepatic ducts connected together by intermediate branches in the transverse fissure of the liver, as E. H. Weber long ago demonstrated, but the branches coming off from these communicate with each other as well as with the trunks from which they come off. These branches are very numerous, and form an intimate network of irregular branched ducts. Similar communications occur between the branches in the portal canals;

but they are not so numerous. This arrangement occurs to a less extent in the dog and in the calf, but it is not present in all animals. The author has not been able to demonstrate it in the pig, seal, rabbit, horse, cat and monkey, although he is not prepared to say that, absolutely, no communications take place between the ducts near their origin, in these animals.

Of the glands of the ducts.—The so-called glands are small cavities of a rounded or oval form, or more or less branched, which communicate with the cavity of the duct by a very constricted neck. The simple glands are for the most part situated in the coats of the ducts, so that, when injected, they scarcely project beyond the external surface. These cavities or glands are most easily demonstrated in the pig.

When a small duct from the human liver is laid open, two lines of orifices are seen opening upon the internal surface, as Kiernan described. The great majority of these, however, are not the openings of glands, but almost all of them are the orifices of branches of the duct which communicate with each other in its coats, or just at the point where they leave it. Very few of them are the openings of cæcal cavities, which are very rare in the smaller ducts of the human subject.

Vasa aberrantia.—There are many curious branches of communication between the ducts in the transverse fissure of the liver, which have been well named "vasa aberrantia" by Weber. Theile looks upon all these ducts as anastomosing mucus-glands. The author has seen these ducts in the portal canals, down to those not more than one-eighth of an inch in diameter. They present the same characters as the branches in the transverse fissure, but are not so numerous. The coats of the vasa aberrantia are thinner than those of the ordinary ducts, and, like them, are lined with epithelium, principally of a subcolumnar form. These branches are always surrounded by areolar tissue, in which lymphatics are very numerous. The arrangement of the vessels about the vasa aberrantia is peculiar. The arteries and veins form a network, and each small branch of the artery lies between two branches of the vein, which communicate with each other at frequent intervals by numerous transverse branches, some of which pass over and some under the artery. The author observes that this beautiful arrangement of the vessels

occurs in the gall-bladder, in the transverse fissure, and in the portal canals. This disposition of the veins has the effect of ensuring free circulation through them under different conditions, as when they are stretched or compressed.

The vasa aberrantia in the transverse fissure of the adult human liver are nearer to the branch of the portal vein than to the hepatic substance, and can be readily removed without any of the latter. A few small straight branches are sometimes observed to come off from the vasa aberrantia and to enter the hepatic substance. In the fœtus, on the other hand, the vasa aberrantia are fewer in number, their course generally is more direct, they lie so close to the hepatic tissue that they cannot be removed unless a portion of the latter is taken away with them, and very many of the branches can be traced into the hepatic substance.

The author regards the vasa aberrantia in the adult liver in the light of altered secreting tubes, and believes that at one time they formed a part of the secreting structure of the liver. At the termination of intrauterine life the portal vein increases in size, and the pressure thus produced may account for the gradual wasting and partial disappearance of the hepatic substance closely surrounding it. In the very thin edge of a horse's liver, which consisted principally of areolar tissue, the gradual alteration of the ducts and ultimate complete disappearance of the secreting cells was traced. Upon the surface of the portal vein in the rabbit's liver the transitional stages between the compact lobule of secreting structure and the branches of the vasa aberrantia have been well seen.

Function of the glands and vasa aberrantia.—It has always been considered that the office of the ducts was to secrete the mucus of the bile, and a similar function was assigned to the vasa aberrantia by Theile. It seems to the author that a cavity communicating with a tube by a neck of less than $\frac{1}{5000}$th of an inch in diameter, cannot be well adapted for pouring out a viscid, tenacious mucus. If these cavities contained mucus, the injection would not enter them so readily as it does, nor is it easy to conceive how the mucus poured out by these little glands would become thoroughly mixed with the bile as it passes along the ducts. Again, the bile of the pig, in which animal these glands are very abundant, does not contain more mucus than the bile of the rabbit, in which they are few

in number and only found on the largest branches of the duct. The vasa aberrantia do not possess any characters which, in the opinion of the author, justify the inference of their being mucus-glands. He regards the little cavities in the coats of the ducts (glands of the ducts) and the vasa aberrantia as reservoirs for containing bile, whilst it becomes inspissated and undergoes other changes. By these cavities in the ducts with thick walls, the bile is brought into close relation with the vessels which ramify so abundantly upon the external surface of the ducts.

Of the finest branches of the duct, and of their connexion with the cell-containing network.

Mammalia.—In well-injected preparations the smallest branches of the duct can be readily traced up to the secreting cells of the lobules. In most Mammalia, but not in the pig, a few of the finest branches of the duct can be followed for some distance beneath the surface of the lobule. These branches appear to lie amongst the secreting cells, but are not connected with them. They become continuous with tubes of the cell-containing network at a deeper part, while those secreting tubes nearer the surface of the lobule are connected with branches of the duct which do not penetrate.

In many animals, particularly in the rabbit, and to a less extent in man and in the dog, the smallest branches of the duct are connected together so as to form a network, which is continuous with that in which the secreting cells lie.

In the pig, the small ducts are, as it were, applied to the surface of the lobule; from these smaller branches come off, which penetrate the lobule and are immediately connected with an intimate network, which lies partly in the capsule of the lobule itself. This network is continuous with, and may be looked upon as the most superficial portion of, the cell-containing network. In a perfectly normal state it contains only oil-globules and granular matter; but when the liver is fatty, it is found to contain liver-cells loaded with oil. From such a liver the author has a very beautiful preparation, in which the continuity of the very narrow duct with the wide tubes of the network, distended with large cells containing oil, can be well seen. The duct and the tubes in which the secreting cells lie, both contain a little injection.

The author has succeeded in demonstrating the communication between the ducts and cell-containing network in several mammalian animals, as well as in the human subject, by injecting the ducts in the manner described. Of these, the seal, hedgehog, rabbit and Guinea-pig have afforded the best specimens.

In *Birds*, the continuity in injected specimens has been traced in the common fowl and in the turkey. The quantity of epithelium in the ducts of birds forms a great obstacle to the passage of the injection, and from their extreme tenuity, the capillaries do not bear the preliminary injection of much water.

Reptiles.—The author has seen the continuity between the ducts and cell-containing network, in an uninjected preparation of the newt's liver, and in an injected liver of the adder.

Fishes.—In consequence of the very fatty nature of the liver of fishes, it was found to be very difficult to harden it sufficiently to cut thin sections. The frequent presence of entozoa and their ova, renders it difficult to inject the ducts. The author succeeded in injecting the ducts and part of the cell-containing network in the sturgeon and in the Lophius, and in one instance, those of the very fatty liver of the cod. The continuity was also traced in an uninjected liver of the common flounder. The injection often passes a certain distance into the finer ducts of fishes, but cannot be forced into the cell-containing network. In this way the appearance of blind terminations to the ducts is produced, as the continuity of the tube cannot be traced beyond the point at which the injection stops.

The continuity of the finest ducts with the cell-containing network has been demonstrated in all classes of Vertebrata, both in injected and also in uninjected specimens. In all the livers of vertebrate animals which have been examined, the duct becomes much narrowed at the point where it joins the network in which the cells lie. The arrangement of the small ducts varies somewhat in different animals. Sometimes a network of minute ducts is formed, which is continuous with that in which the cells lie. In other instances the communications between these terminal ducts are very few in number, or are altogether absent. Upon the latter point the author does not express himself positively, as he is sure that in the most perfect injection which he has been able to make, the whole of

the numerous branches of the minute ducts have not been injected; and from observations upon these specimens alone, he feels that only a very imperfect idea can be formed of their number or of their arrangement.

Diameter of the ducts.—A table is given, showing the thickness of the coats of the ducts in different parts of their course. The walls of the smallest ducts are composed entirely of basement membrane, and are often not more than the $\frac{1}{5000}$th of an inch in diameter in the uninjected state. In the pig, the diameter of the smallest ducts containing injection was about the $\frac{1}{3000}$th of an inch; in the human subject, about the $\frac{1}{2500}$th; in the seal, $\frac{1}{3000}$th; and in some fishes not more than the $\frac{1}{5000}$th of an inch. It may be remarked, that this narrowing of the excretory duct, just before it becomes continuous with the secreting portion of the organ, is seen in the kidney and in other glands.

Epithelium of the small ducts.—The epithelium lining the smallest ducts presents very similar characters in different animals. The small cells are for the most part oval or circular in form; sometimes they are angular, which probably results from pressure or stretching of the ducts in the preparation of the specimen; sometimes the smallest ducts appear to be entirely filled with epithelium; in other instances the cells are very sparingly and irregularly scattered over the interior of the tube, while frequently no cells whatever can be distinguished.

The author believes that, in a perfectly normal state, the minute ducts are lined by a single layer of delicate epithelial cells.

This ductal epithelium does not pass gradually into the secreting epithelium, but ceases at the point where the latter begins. Hepatic cells are sometimes seen in tubes lined with this ductal epithelium, but probably their presence is the result of accident. In these cases the ducts are of course much stretched or dilated *.

With reference to the relation of the ductal epithelium to the secreting cells of the liver, the author observes that a very similar arrangement occurs in the gastric glands. The secreting epithelium is alone found in the lower part of the gland (stomach tube), while the ductal portion of the gland is lined with columnar epithelium.

* Mr. Wharton Jones has also seen hepatic cells in the small ducts.—*Phil. Trans.*

The secreting cells appear to occupy the entire cavity of the tube, and are not arranged in any order; so that the secretion, having escaped from the cells, must pass off towards the duct by the slight interstices between them. A similar disposition of the secreting epithelial cells occurs, but in a less remarkable degree, in some other glands; as the pancreas, lacteal, sebaceous, and sweat glands.

The conclusions to which the author has arrived may be summed up as follows:—

1. That the liver of vertebrate animals essentially consists of two solid tubular networks mutually adapted to each other. One of these networks contains the liver-cells, and the other the blood.

2. The cell-containing network is continuous with the ducts. The small delicate epithelial cells lining the latter channels contrast remarkably with the large secreting cells, which are not arranged in any definite manner within the tubes of the network.

3. The duct is many times narrower than the tubular network at the point where it becomes continuous with it.

4. Injection passes sometimes on one, and sometimes on the other side of the tube, or between the cells, when two or more lie across the tube. Often, a cell becomes completely surrounded with injection. As injection can thus be made to pass readily *from* the ducts into the network and around the cells, it follows that there can be no obstacle to the passage of the bile along the same channels in the opposite (its natural) direction.

5. In some animals, the most minute ducts are directly connected with the tubes of the cell-containing network; of these branches, some pass amongst the most superficial meshes to join the network at a deeper part. In other animals the finest ducts first form a network which is continuous with that containing the liver-cells.

6. The interlobular ducts do not anastomose, but the branches coming off from the trunk are often connected with each other, as well as with the parent trunk, near their origin from it.

7. The walls of the smallest ducts are composed of basement membrane only. The thick complex coat of the larger ducts contains within it small cavities (the so-called glands of the ducts), by means of which the bile in these ducts would be brought into close proximity with the arteries, veins and lymphatics, which are very abundant wherever the ducts ramify.

8. The office of the vasa aberrantia, which are so numerous in the transverse fissure of the human liver and in the larger portal canals, appears to be similar to that of the cavities in the walls of the ducts. It is worthy of remark, that the network of vessels ramifying so abundantly in the coats of the gall-bladder, in the transverse fissure, and in the larger portal canals, are arranged in a similar manner, each branch of artery being accompanied by two branches of the vein.

9. The liver is therefore a true gland, consisting of a formative portion and a system of excretory ducts directly continuous with it. The secreting cells lie within a delicate tubular network of basement membrane, through the thin walls of which they draw from the blood the materials of their secretion.

XI. "Experimental Researches on the Movement of Atmospheric Air in Tubes." By W. D. Chowne, M.D. Communicated by John Bishop, Esq., F.R.S. Received June 14, 1855.

In the year 1847, the author of this paper made numerous experiments for the purpose of ascertaining what are the conditions under which atmospheric air is placed with regard to motion or rest, when within a vertical tube having one extremity communicating within the interior of a building, and the other in the open atmosphere.

The paper now submitted to the Royal Society contains the results of investigations undertaken in the year 1853 and continued to the present time, to ascertain whether the ordinary state of atmospheric air contained in a vertical cylindrical tube, open at both ends, and placed in the still atmosphere of a closed room, is one of rest or of motion; and if of motion, to investigate the influences of certain changes in the condition of the atmosphere which either produce, promote, retard, or arrest the movement.

He demonstrates, by a series of experiments, that when a tube, open at both ends, is placed in a vertical position, every precaution being taken to exclude all extraneous causes of movement in the surrounding atmosphere, an upward current of air is almost imme-

diately established, and continued so long as these conditions are maintained.

The experiments were made in a room 12 feet square by 8 feet 6 inches high; the windows and chimney being carefully secured, and all crevices closed, by pasting paper over them, the floor carpeted, the door double, and the inner door surrounded with list. The outer wall, having a north aspect, was so sheltered by surrounding buildings that the direct rays of the sun never fell upon the window. Discs of delicate tissue-paper were suspended in several parts of the room, to indicate currents of air, if any existed, and observations were taken only when these were perfectly quiescent.

Mason's hygrometer was first employed in these experiments, to test the presence of a current of air in the tube; on the principle that as evaporation produces cold, and as evaporation is increased by a current of air, the wet-bulb thermometer would show a greater depression if any current existed, than if the air were perfectly quiescent within the tube. The tube was placed in the middle of the room, and isolated from the floor by a cylinder of thick glass laid under it.

It was found that in ninety-one observations of the hygrometer, suspended in the free air of the room, the mean depression of the wet-bulb thermometer was $3^\circ{\cdot}9$ Fahr., while in ninety corresponding observations, with the hygrometer at the lower aperture of the tube, the mean depression was increased to $4^\circ{\cdot}9$ Fahr., clearly indicating the existence of a current of air within the tube.

Partial closure of the upper orifice of the tube, by placing a piece of fine muslin upon it, produced a sensible influence on the hygrometer. In seventeen observations with the tube thus partially obstructed, the mean depression was $2^\circ{\cdot}5$ Fahr.; but in an equal number of comparative observations, with the tube perfectly free, the mean depression was increased to $3^\circ{\cdot}12$ Fahr.; showing a considerable diminution of the force of the current within the tube, as a result of the partial obstruction of its upper aperture.

Similar comparative observations, with the hygrometer placed at the upper aperture of the tube, yielded similar results.

In these experiments the lower extremity of the vertical tube was bent thrice at right angles*, for convenience in making the obser-

* The tubes used in these experiments were bent either at their lower or upper extremities for convenience merely.

vations, and it appeared desirable to ascertain what influence the long branch of the siphon-like tube had in the production of the current. For this purpose the long vertical tube was made moveable, so that the apparatus could be alternately converted into a siphon with equal limbs 4 inches in length, or one with a short leg of 4 inches, and a long one of 96 inches. In twelve observations, when the long leg was inserted, the mean depression of the hygrometer was 2°·5 Fahr.; when the limbs were of equal length, 2°·25 Fahr.

Considering it possible that the current of air existing in the tube might have sufficient force to move a light body delicately suspended in its track, an elbow was inserted into the upper orifice of the tube, to which a piece of glass tube of the same diameter was adapted, 6 inches in length, and a disc of tissue-paper, weighing one grain, which nearly occupied the area of the tube, was delicately suspended by a hair, at right angles to the axis of the tube. A slide valve was so adapted to the lower orifice, that this aperture could be opened or closed without entering the room. The air of the room being quiescent, it was found that when the slide valve closed the lower orifice of the tube, the disc of tissue paper remained perfectly quiescent; but that when the slide valve was withdrawn, leaving the lower orifice open, oscillations of the paper occurred, and it was projected at a small angle towards the upper orifice of the tube, demonstrating the existence of a feeble current of air through the apparatus.

The preceding experiment having proved the existence of a current of air within the tube, of sufficient force to move a light body, the author next proceeded to ascertain the velocity of the current by means of an anemometer, in the form of an horizontal fly-disc, suspended within the lower orifice of a tube, bent twice at right angles below. The revolving disc was made of a circular piece of stout writing-paper, cut into twenty-four equal segments, from the circumference to near the centre, each of the segments being afterwards inclined at an angle of twenty-five degrees*, like the vanes of a windmill; so that when properly suspended, a current of air entering the lower orifice of the tube would cause the disc to revolve from right to left. The disc was suspended in the same manner as the needle of the mariner's compass, and by the same means.

* A nearer approach to an angle of 45 would have crippled the paper, so that it would not have preserved the horizontal position.

When the apparatus was arranged, the door of the room closed, and the atmosphere in a quiescent state, it was found that a constant regular rotation of the disc was established, and kept up by the upward current of air through the apparatus, and continued so long as the atmosphere of the room was quiet; but that agitations of the surrounding air either rendered the rotation uncertain, or reversed it.

Having thus ascertained that the current of air within the vertical tube possessed sufficient force to cause the rotation of a lightly suspended fly-disc, the question arose, what influence elongation or shortening of the tube would exert on the velocity of the current. For this purpose three tubes of precisely similar construction, but with long limbs of 12, 24, and 48 inches respectively, were fitted as before with fly-discs, and placed near each other in the centre of the room.

In nineteen observations, the number of revolutions in the tube, with a long limb of 12 inches, varied from 0·75 to 4·5 per minute; in that with a long limb of 24 inches, from 1·5 to 9·0, and in that with the long limb of 48 inches, from 3·75 to 14·0 per minute. The gross number of revolutions in the three tubes, in the nineteen observations, were respectively 51·25, 111·25, and 199·75; and the mean revolutions per minute, 2·697, 5·855, 10·513, which, allowing for errors of observation, yield the ratios 1, 2, 4 nearly; so that it may be said that the velocity of the revolutions is in a direct ratio to the lengths of the vertical tubes.

The influence of the conoidal form of the tube being suggested by Dr. Roget as worthy of investigation, a tube, 96 inches long by 3 inches diameter below and 6 inches above, was fitted to a rectangular tube containing the rotating disc. Another tube of the same length, 3 inches in diameter throughout, was placed near the conical tube as a term of comparison. The revolutions of the disc in the conical tube were more rapid than in that of uniform diameter, in the proportion of 8·8 to 3·0. When the position of the cone was reversed, and the entrance and exit orifices were equal, the revolutions still continued more rapid than in the tube of uniform diameter.

To determine the influence of the area of the tube on the velocity of the current, four tubes, 96 inches in length in the long, and

4 inches in the short branch, but varying in diameter, were placed in the room near each other and simultaneously observed.

In a tube of 3 inches uniform diameter, the revolutions were 3·0 per minute; in one of 5 inches 9·15, and in one of 6·75 inches 13·15; their respective areas being 7·065, 15·708, and 21·205. In the conical tube on its base, whose area was 14·529, the revolutions were 8·8 per minute. It would seem, then, that the velocity has relation rather to the mean area of the tube than to that of the entrance and exit orifices, as the latter were the same in the tube of 3 inches uniform diameter, and in the conical tube on its base, while the revolutions of the disc were 3·0 per minute in the former, and 8·8 per minute in the latter. When the exit orifice of the tube of 6·75 inches diameter was reduced to 3·5 inches, the rapidity of the revolutions was reduced only about 10 per cent.

The influence of temperature in accelerating or retarding the currents through the tubes next engaged the author's attention; but before entering into direct experiments, he found by very numerous observations, that on some occasions no appreciable difference could be observed in the temperature of the atmosphere of the room near the floor and the ceiling, while on others there was a mean excess of $0^{\circ}\cdot 17$ Fahr. near the ceiling without causing any perceptible difference in the velocity of the revolutions of the discs. In forty comparative observations of the temperature of the external surface of the tube and of the surrounding air, that of the tube was 0·09 higher; in twenty-three it was equal, but in only five was it lower than the surrounding air.

Of thirty-six comparative observations of the temperature of the air within, and external to the tube, by a delicate mercurial thermometer, it was found to be slightly higher within the tube in twenty-seven, and in the remaining nine it was equal, but never lower than that of the external air. The greatest excess was $0^{\circ}\cdot 4$ Fahr., and the mean excess $0^{\circ}\cdot 14$ Fahr.

The accuracy of these results was tested by an extremely delicate differential thermometer, which also indicated a minute excess of temperature within the tube; the author is led however to infer, that in the thirty-six observations the mean of $0^{\circ}\cdot 14$ is rather above the true excess; taking this however to be the exact amount, and as the atmospheric air is increased only $\frac{1}{480}$ of its volume, for every

degree of Fahrenheit's thermometer, we shall have $\frac{14}{100}$ of $\frac{1}{480}$ for the increase of the whole bulk of that in the tube.

The disc continued to rotate while the thermometer indicated equal temperature in the tube and external to it, in eight of the cases, and was arrested by an accident in the ninth.

In another experiment, when the lower orifice of the tube was alternately closed and opened by a valve, the temperature appeared under both circumstances to be the same; hence, if we assume that a minute excess of temperature of the air within the tube, over that of the air external to it, exists, yet the experiment shows that it is not attributable to any heat being disengaged by the movement of the air itself.

Increase and decrease of the temperature of the room exercised a considerable influence on the velocity of the rotations of the discs, which increased as the day advanced, and declined as the temperature fell towards evening, although the direct rays of the sun never fell upon the window of the room.

Partial exclusion of light, by a blind covering the whole window, produced a considerable reduction in the velocity of the rotations of the discs, but a screen of a foot in breadth, interposed between the window and an individual tube, merely reduced the velocity of the rotations from 12·5 to 11·0 per minute.

The influence of reduction of temperature of the long branch of the tube, by placing around it two coils of wet tape*, reduced the revolutions of the disc from 4·0 to 1·75 per minute; a third reduced the revolutions to 1·0; a fourth to 0·5; and a fifth caused complete cessation.

To ascertain the influence of the abstraction of aqueous vapour on the rotation of the discs, a shallow vessel, containing strong sulphuric acid, was placed, at the suggestion of the Rev. Dr. Booth, immediately below the disc in the short branch of the tube. After the lapse of thirty minutes, the rotation had ceased altogether; at the commencement the disc was rotating at the usual rate. The same vessel, placed in the tube without the sulphuric acid, had no effect on the rotation.

In another experiment a bell-glass was suspended over the short branch of the tube, so that the short branch projected into it, and a

* Half an inch broad, and not so wet that any of the water ran away from it.

saucer, containing concentrated sulphuric acid, wasal so placed under the bell-glass, on a level with the orifice of the tube. The rotations of the disc were accelerated by placing the warm hand for a few seconds in contact with the long branch of the tube; but at the end of five minutes after it was withdrawn, and the room left and closed, the disc had ceased to rotate.

To determine the influence of partial abstraction of aqueous vapour from the entire atmosphere of the room on the velocity of the rotations, the three tubes, with long limbs of 12, 24, and 48 inches, employed in a previous experiment, were placed near each other, and three bushels of quicklime were spread in shallow vessels on the floor and other parts of the room. Before the lime was placed, the disc in the 12-inch tube was revolving at the rate of 0·75 per minute; that in the 24-inch tube at 2·0; and that in the 48-inch tube at 4·0 per minute. At the end of fifty minutes the rotation had ceased in the 12-inch tube, and was reduced to 1·75, and 3·5 in the 24- and 48-inch tubes. After seventy minutes, rotation had ceased in the 24-inch tube, and was reduced to 3·75 in the 48-inch tube. Finally, after ninety minutes, the rotations in the 48-inch tube were reduced to 2·75 per minute.

Similar reductions in velocity were observed after the removal and reintroduction of the quicklime in a second and third series of observations. Thus in all these experiments the rotations in the 12- and 24-inch tubes entirely ceased; and those in the 48-inch tube, although continued, were much diminished; a result most probably attributable to the greater quantity of aqueous vapour remaining in the upper strata of the air in the room.

The mean depression of the wet-bulb thermometer, the hygrometer being placed 48 inches above the floor, and the lime being absent, was 3·2; when the lime was present, 3·4. When the hygrometer was on the floor the depression of the wet bulb was 3·5.

As the abstraction of aqueous vapour from the atmosphere diminished and even abolished the currents of air within the tubes, it was to be expected that increase of vapour in the atmosphere would produce the contrary effect, and accelerate the currents and the corresponding revolutions of the discs, and the following results coincide with that expectation.

In the first experiment, the tube and bell-glass previously described

were employed, but substituting folds of damp linen for the saucer of sulphuric acid, so as fully to charge the air in the bell-glass with vapour, the rotations rapidly rose from 4·0 to 17 or 18 per minute.

But as the cold produced by the evaporation of the water in this experiment might be a source of fallacy, an arrangement was made to supply the bell-glass with air, previously charged with vapour, formed at a distance of 5 feet from the glass. The rapidity of the revolutions was however still considerably increased.

Augmentation of the quantity of aqueous vapour, in the general atmosphere of the room, by spreading wet cloths on the floor and other parts, also produced increase in the rapidity of the rotations, though to a small extent.

These experiments* would seem to demonstrate that the ordinary condition of atmospheric air within vertical tubes, open at both extremities, is one of continual upward movement.

If the atmosphere were a strictly homogeneous elastic fluid, and in a state of perfect equilibrium, any portion of it contained in a vertical tube would of course be perfectly stationary unless some adventitious cause produced disturbance of its equilibrium. But our atmosphere being a mixed fluid, and the aqueous vapour being of a much lower specific gravity at all atmospheric temperatures than the compound of which it forms a part, it is constantly rising within a tube, as in the free air; entering at the lower, and making its exit at the upper orifice of the tube.

The experiments appear further to demonstrate, that the presence of aqueous vapour in the atmosphere is essential to the production of the current within the vertical tubes, since by the abstraction of vapour from the air by quicklime, the rotations of the discs were invariably either diminished or caused to cease; while on the other hand, when the proportion of aqueous vapour in the air was increased, the currents and the rotations of the discs were simultaneously accelerated.

* Throughout the entire series the results were carefully observed during the night, when the atmosphere of the room was free from solar influences. The dry- and the wet-bulb thermometers yielded the same relative differences, and the discs rotated with the same constancy. The night as well as the day observations were continued through all the changes of temperature, from March 1853 to the present time.

In concluding the details of these experiments, the author considers that they all tend to prove the existence of an upward current, under the circumstances described in the commencement of this paper.

They moreover yield a series of results which he hopes the Society will deem to be not without interest.

These results show it to be probable, if not certain, that the ordinary temperature of air within tubes, under the circumstances in which these were placed, is higher than of that external to them, all other relations of the tubes and surrounding objects being the same; they also show that in eight instances, when the thermometers indicated an equality of temperature, within and external to the tube, the rotations of the discs still continued; and that when four coils of tape, moistened with water, were applied round the external surface of the tube, the rotations of the disc did not wholly cease.

They also show, that when the atmosphere of the room, in which the tubes were immersed, contained a larger or smaller proportion of aqueous vapour, all other things being equal, the discs revolved with more or less velocity; but that when the atmosphere was deprived in a great degree of aqueous vapour by the presence of quicklime, the thermometric state in all other respects remaining the same, the revolutions of the discs ceased.

Adverting to the indications cited above, of a minute excess of temperature in the interior of the tubes, and assuming that even that slight excess would be sufficient to rotate the discs, still the rotations diminished or ceased in proportion as the aqueous vapour was withdrawn.

Any increase of temperature which might have been produced by the quicklime would have had a tendency rather to increase than diminish the revolutions of the discs, but we have seen that the abstraction of the vapour entirely arrested their rotation.

With regard to the specific influence of each of the circumstances and agents most probably concerned in producing the phenomena described above, such as protection of the air within the tube from lateral expansion and mechanical agitations, to which the external air is exposed; gaseous diffusion; the unequal specific gravity of air and vapour; and the subtle operations of temperature at all times, the author is fully conscious that he has not ascertained their respective values.

He is also conscious that the phenomena themselves are the chief ground on which he can rest a claim for originality, and that the explanation of them may be better treated by those who are more accustomed to deal with similar researches.

In the course of these experiments the author has been especially indebted for many valuable suggestions to the Rev. Dr. Booth, Dr. Roget, Professor Sharpey, and Mr. Bishop; and he is also under obligations to Professor Stokes and to Mr. Brooke.

XII. "On the Constitution and Properties of Ozone." By Thomas Andrews, M.D., F.R.S., Professor of Chemistry in Queen's College, Belfast. Received May 16, 1855.

The conflicting views which have so long existed as to the true constitution of ozone, induced the author to undertake a careful investigation of the subject, particularly as he had reason to doubt the accuracy of the only quantitative experiments which have yet been made to elucidate this difficult question. According to the experiments referred to, two substances have been confounded under the name of ozone, one a compound body having the formula HO_3, the other an allotropic variety of oxygen. To ascertain whether, in conformity with this statement, ozone obtained in the electrolysis of water contains hydrogen as a constituent, the author made two series of experiments. In the first series, he followed nearly the same method of investigation by which its compound nature is supposed to have been established, but modified so as to avoid a source of error, which, if neglected, vitiates altogether the results. Electrolytic oxygen, unless very great precautions be taken, is always accompanied by a small but appreciable quantity of carbonic acid, which is liable to be partially absorbed by the potassa set free when a neutral solution of iodide of potassium is decomposed by ozone. By adding a little hydrochloric acid to the solution of iodide of potassium before the commencement of each experiment, this error may be avoided.

The method of performing the experiment was to conduct a stream of electrolytic oxygen through a compound apparatus previously

weighed, which contained on one side an acid solution of iodide of potassium, and on the other sulphuric acid; the former to decompose the ozone, the latter to prevent the escape of moisture. The increase in weight of this apparatus gave the entire weight of the ozone; the iodine set free, when reduced to its equivalent in oxygen, the weight of the active oxygen. The precautions to be taken in conducting this experiment are fully described in the communication.

The following are the numerical results of five experiments performed according to the above method:—

Volume of electrolytic oxygen.	Increase in weight of compound apparatus.	Active oxygen deduced from iodine set free.
litres.	grm.	grm.
10·20	0·0379	0·0386
2·72	0·0107	0·0100
2·86	0·0154	0·0138
6·45	0·0288	0·0281
6·80	0·0251	0·0273
Total	0·1179	0·1178

The agreement in these numbers proves that the active oxygen is exactly equal to the entire weight of the ozone, and is therefore identical with it.

In the next series of experiments the author shows that no water is produced in the decomposition of electrolytic ozone by heat. Large quantities of electrolytic oxygen, containing from 38 to 27 milligrammes of ozone, were decomposed by heat, but no water was obtained in a weighed absorption apparatus, in which the gas was exposed, not only to the action of sulphuric acid, but was also passed through a tube containing anhydrous phosphoric acid.

Having confirmed by new experiments the fact that ozone is formed by the action of the electrical spark on pure and dry oxygen, the author proceeds to institute a comparison between the properties of ozone derived from different sources. These he finds to be in every respect the same. Thus ozone, however prepared, is destroyed, or rather converted into ordinary oxygen, by exposure to a temperature of about 237° C., and catalytically, by being passed over peroxide of manganese, no water being formed in either case; it is not absorbed by water, but when sufficiently diluted with other

gases, is destroyed by agitation with a large quantity of water; it is also, contrary to the common statements, destroyed by being agitated with lime-water and baryta-water, provided a sufficient quantity of those solutions be used; it has always the same peculiar odour; it bleaches without producing previously an acid reaction; it oxidizes in all cases the same bodies, &c.

From the whole investigation the author draws the conclusion, "that ozone, from whatever source derived, is one and the same substance, and is not a compound body, but oxygen in an altered or allotropic condition."

XIII. "On Rubian and its Products of Decomposition."—Part III. By Edward Schunck, F.R.S. Received June 13, 1855.

Combined Action of Alkalies and Oxygen on Rubian.

In the preceding part of this paper the author has shown that the action of alkalies is essentially the same as that of acids on rubian, the only difference being that the rubianine produced by acids is replaced by rubiadine when alkalies are employed. Now though this is in all cases the final result of the action of alkalies, there still remained a possibility of the existence of bodies intermediate between rubian and the final products of decomposition. Such bodies do in reality exist, but their formation is dependent, in part at least, on the simultaneous action of oxygen.

When alkalies or alkaline earths, as potash, soda, ammonia, baryta or lime, or the bicarbonates of baryta or lime are added to a watery solution of rubian, and the solution is exposed to the air, oxygen is absorbed, and three distinct bodies are formed, to which the author has given the names of *Rubianic Acid, Rubidehydran* and *Rubihydran.* The method of separating these bodies and obtaining them in a state of purity is fully detailed. Even oxide of lead is a sufficiently strong base to cause rubian to undergo this process of decomposition in the presence of oxygen. From this cause the lead compound of rubian, after being exposed for some time to the atmosphere, no longer contains unchanged rubian, but products of its decomposition; and hence also it follows that in the processes pro-

posed by Kuhlmann, Berzelius and others for the preparation of the so-called xanthine, and in that of Rochleder for the preparation of his ruberythric acid, all of which depend on the use of alkaline earths or basic acetate of lead, products of decomposition of rubian must in every case be formed. Besides the substances just mentioned, a little acetic acid, and sometimes also rubiadine, and sugar in small quantities are formed. Whether the first is an essential product of decomposition or not, the author leaves undecided; but the other two he considers as decidedly secondary products, resulting from the decomposition of rubidehydran or rubihydran.

Rubianic acid is soluble in water and alcohol, but not in ether. It crystallises in lemon-yellow silky needles. Its watery solution has a distinctly but not strongly bitter taste. When heated it yields a sublimate of alizarine. By the action of strong acids, as well as of caustic alkalies and of erythrozym, it is decomposed into alizarine and sugar. Its compounds with potash and ammonia crystallize in small puce-coloured needles. The soda salt is deposited from the watery solution as a mass of small red spherical grains, which are very sparingly soluble in water. These compounds possess very little stability. The baryta and lime salts are obtained by double decomposition as red precipitates. The acid is completely precipitated from its watery solution by basic acetate of lead, the precipitate being red, and resembling that produced by the same salt in a solution of rubian.

The composition of rubianic acid is expressed by the formula

$$C_{52}H_{29}O_{27},$$

that of the potash salt by

$$C_{52}H_{28}O_{26}+KO,$$

that of the neutral baryta salt by

$$C_{52}H_{28}O_{26}+BaO+HO.$$

The conversion of the acid into alizarine and sugar is symbolized by the following equation:—

$$C_{52}H_{29}O_{27}+5HO=2C_{14}H_5O_4+2C_{12}H_{12}O_{12}.$$

In order to leave no doubt regarding the correctness of the formula, the author determined the quantity of alizarine formed by the decomposition of the acid. According to calculation, 100 parts of acid should yield 43·44 parts of dry alizarine. In two experiments the author obtained 42·47 and 45·17 per cent. of alizarine.

The presence of oxygen is necessary for the formation of this acid. If a watery solution of rubian made alkaline with soda or baryta be kept out of contact with the air, no rubianic acid is produced, whereas an abundance of the latter is obtained from the same solution on exposure to the atmosphere. The manner in which it is formed from rubian may be represented by the following equation :—

$$C_{56}H_{34}O_{30}+10O=C_{52}H_{29}O_{27}+4CO_2+5HO.$$

The author considers this as the first known instance of a body, belonging to the class of glucosides, or conjugate compounds containing sugar, having been observed to form by a process of oxidation.

The author thinks it probable that this acid and the ruberythric of Rochleder are identical, but the description given of the latter by the discoverer is not sufficiently minute to enable him to come to a decision on this point. Rochleder has moreover given a very different composition for his acid. Until the properties and composition of the latter have been more accurately investigated, the author prefers considering the two acids as distinct. If they are identical, then Rochleder has merely committed the common error of mistaking a product for an educt.

Rubidehydran and rubihydran have both properties very nearly resembling those of rubian, from which they can only with difficulty be distinguished. Neither of them however is capable of yielding rubianic acid when treated in the same way as rubian. The composition of rubidehydran is expressed by the formula

$$C_{56}H_{32}O_{28},$$

that of rubihydran by

$$C_{56}H_{39}O_{35}.$$

The former contains therefore the elements of 2 equivs. of water less the latter, the elements of 5 equivs. of water more than rubian. Both substances give, when decomposed by strong acids, the same products, viz. alizarine, rubiretine, verantine, rubiadine and sugar. The products formed by acids are therefore the same as those produced from rubian by alkalies, which renders it probable that the latter, when acted on by alkalies, is first converted into rubidehydran or rubihydran, or both.

Rubiadine may be obtained from rubihydran in large quantities

and in a state of great purity, and the author had thus an opportunity of examining the properties and composition of this substance more accurately than heretofore. From the new analyses which he made he infers that its formula is

$$C_{32}H_{13}O_{9},$$

which differs from

$$C_{32}H_{12}O_{8},$$

the one formerly given, by 1 equiv. of water.

Action of Chlorine on Rubian.

When chlorine gas is passed through a watery solution of rubian, the solution deposits lemon-yellow or orange-colour flocks and becomes colourless. The flocks consist of a peculiar substance, which the author calls *Chlororubian*. The liquid contains sugar. Chlororubian crystallizes from its solution in alcohol in small orange-coloured needles. It is soluble in boiling water, but not as easily as in alcohol. On being heated, however carefully, it is decomposed. It is dissolved by caustic and carbonated alkalies, forming blood-red solutions. The baryta and lime compounds are red and insoluble. The watery solution produces with basic acetate of lead a light red precipitate. The author gives for the chlororubian the formula

$$C_{44}H_{27}ClO_{24}.$$

Its formation from rubian is represented by the following equation :—

$$C_{56}H_{34}O_{30}+6HO+2Cl=C_{44}H_{27}ClO_{24}+C_{12}H_{12}O_{12}+2ClH.$$

Chlororubian is decomposed by strong acids and splits up into sugar and another body, which has the formula

$$C_{32}H_{12}ClO_{9},$$

and which, in consequence of the relation in which it stands to rubiadine, the author calls *Chlororubiadine*. The manner in which this process of decomposition takes place will be seen from the following equation :—

$$C_{44}H_{27}ClO_{24}=C_{32}H_{12}ClO_{9}+C_{12}H_{12}O_{12}+3HO.$$

Chlororubiadine is soluble in alcohol. The boiling solution deposits it on cooling in yellow shining needles. It is insoluble in boiling water. It is not decomposed by dilute nitric acid or on boiling, but nitric acid of sp. gr. 1·52 dissolves, and on boiling

decomposes it, and nitrate of silver now gives a precipitate of chloride of silver. Chlororubiadine dissolves in caustic and carbonated alkalies with a red colour. With baryta it gives a compound, crystallizing in long red needles. The author did not succeed in converting rubiadine into chlororubiadine, nor, on the other hand, was he able to substitute the chlorine of the latter by hydrogen, and thus form rubiadine.

The sugar which is formed from chlororubian, together with chlororubiadine, may be obtained in a crystallized state, when it has the properties and composition of crystallised grape-sugar.

When chlororubian is treated with caustic soda, the chlorine is entirely separated, forming chloride of sodium. The other products of decomposition are verantine, rubiretine, a body resembling rubiadine, sugar, and a yellowish-brown substance insoluble in water, in alcohol, and even in alkalies, the probable formula of which is

$$C_{44}H_{14}O_{12},$$

and for which the author proposes the name of *Oxyrubian,* since it owes its formation to the chlorine of chlororubian being replaced by oxygen.

By the continued action of chlorine chlororubian is converted into a white body, which the author calls *Perchlororubian.* This body is insoluble in water and caustic alkalies, but soluble in alcohol and ether. It crystallizes from the alcoholic solution in colourless, transparent, four-sided tables, exhibiting a beautiful iridescence. When carefully heated it may be entirely volatilized. It is not decomposed by nitric acid of sp. gr. 1·52, even on boiling, but is merely dissolved. Its composition is expressed by the formula

$$C_{44}H_{9}Cl_{9}O_{15}.$$

From these experiments it follows that chlororubian is a conjugate body containing sugar. In this respect it resembles Piria's chlorosalicine.

In all processes of decomposition previously described rubian yields three series of compounds, just as if it consisted of three bodies. When acted on by chlorine, however, it yields only one series of products, which corresponds exactly with one of the three series of the other processes, the bodies belonging to the two other series not making their appearance, even in the form of products of substitution.

XIV. "On the Enumeration of x-edra having an $(x-1)$-gonal Face, and all their Summits Triedral." By the Rev. Thomas P. Kirkman, A.M., Rector of Croft-with-Southworth. Communicated by A. Cayley, Esq., F.R.S. Received June 13, 1855.

The object of the paper is to enumerate the x-edra which have an $(x-1)$-gonal face, and all their summits triedral; or, what is the same thing, to find the number of the x-acra which have an $(x-1)$-edral summit, and all their faces triangular.

Every x-edron having an $(x-1)$-gonal face has at least two triangular faces. Let A be an x-edron having all its summits triedral, and having about its $(x-1)$-gonal face k triangular faces. Suppose all these triangles to become infinitely small; there arises an $(x-k)$-edron B, having an $(x-k-1)$-gonal face, and all its summits triedral. B will have k' triangular faces, k' being not less than two, nor greater than k. And there is no other $(x-k)$-edron but B, which can arise from the vanishing of all the k triangles of A; *i. e.* there is no $(x-k)$-edron but B, from which A can be cut by removing k of the summits of B in such a way as to leave none of its k' triangles untouched.

If we next suppose the k' triangles of B to vanish, there will arise an $(x-k-k')$-edron C, having an $(x-k-k'-1)$-gonal face, all its summits triedral, and k'' triangular faces, k'' not <2, nor $>k'$. And thus we shall at last reduce our x-edron, either to a tetraedron, or to a pentaedron having triedral summits.

All x-edra here considered fall into six varieties, differing in the sequence of the $x-1$ faces that are collateral with the $(x-1)$-gonal base. They are either *irreversible*, as the octaedron 6435443, the seven faces about the base reading differently both backwards and forwards from every face; or *doubly irreversible*, as the heptaedron 543543, whose six faces about the base are a repetition of an irreversible period of three; or *triply irreversible*, as the decaedron 643643643, whose faces exhibit a thrice-repeated irreversible period; or they are *reversible*, *doubly reversible*, or *triply reversible*, as the hexaedron 53443, the enneaedron 63536353, or the heptaedron 535353, exhibiting a single, double, or triple period, all reading

backwards and forwards the same. If P_x be the number of x-edra having an $(x-1)$-gonal base, and all their summits triedral,

$$P_x = I_x + I_x^2 + I_x^3 + R_x + R_x^2 + R_x^3,$$

the symbols on the right denoting the numbers of x-edra of the six varieties that make up P_x.

Each variety is again subdivided according to the number of triangular faces. Thus, if $P(x, k)$ denote the number of x-edra on an $(x-1)$-gonal base, having k triangular faces, and all their summits triedral,

$$P(x,k) = I(x,k) + I^2(x,k) + I^3(x,k) + R(x,k) + R^2(x,k) + R^3(x,k).$$

The number k is not <2, nor $>\frac{x-1}{2}$, and $P_x = \Sigma P(x, k)$, for all values of k.

It is necessary to solve the following

Problem.—To determine the number of $(x+k+l)$-edra, none of which shall be the reflected image of another, that can be made from any x-edron having k triangular faces, by removing $k+l$ of its base-summits, thus adding $k+l$ triangular faces, so that none of its k triangular faces shall remain uncut.

The x-edron is supposed to have an $(x-1)$-gonal face, and all its summits triedral; no edge is to be removed, and $k+l$ not $>x-1$.

When the x-edron, the subject of operation, is *irreversible*, all the resulting $(x+k+l)$-edra will be irreversible. If it is *reversible*, some of them will be reversible and others irreversible; if it is *multiple*, some of them will be, and others will not be, multiple.

If the subject of operation is irreversible, the number required by the problem is

$$ii(x,k,l) = 2^k . \frac{\overline{x-1-k}^{\,l|-1}}{\lfloor\underline{l+1}} - \Sigma_a (2^a - 1) . 2^{k-a} . \frac{k^{a|-1}}{\lfloor\underline{a+1}} . \frac{\overline{x-1-2k}^{\,l-a|-1}}{\lfloor\underline{l-a+1}},$$

taken for all values of a not greater than the least of k and l; i. e. $k-a$ not <0, 0 not $>l-a$.

The complete answer to the problem is expressed by the following equations, in which, of the capitals on the left, the first expresses the result, and the second the subject of operation. That is, $IR^2(x, k, l)$ denotes the number of irreversible $(x+k+l)$-edra having $k+l$ triangular faces about the $(x+k+l-1)$-gonal base, that can

be cut from any doubly reversible x-edron having k triangles about its $(x-1)$-gonal base.

Whenever k or l in the function $ii(x, k, l)$ is not integer, the function, by a geometrical necessity, is to be considered $=0$.

$$\mathrm{II}(x, k, l)=ii(x, k, l),$$
$$\mathrm{II}^2(2x+1, 2k, l)=\tfrac{1}{2}\{ii(2x+1, 2k, l)-ii(x+1, k, \tfrac{1}{2}l)\},$$
$$\mathrm{II}^3(3x+1, 3k, l)=\tfrac{1}{3}\{ii(3x+1, 3k, l)-ii(x+1, k, \tfrac{1}{3}l)\},$$
$$\mathrm{I}^2\mathrm{I}^2(2x+1, 2k, l)=ii(x+1, k, \tfrac{1}{2}l),$$
$$\mathrm{I}^3\mathrm{I}^3(3x+1, 3k, l)=ii(x+1, k, \tfrac{1}{3}l);$$

$$\mathrm{RR}(2x+1, 2k, l)=ii(x+1, k, \tfrac{1}{2}l),$$
$$\mathrm{RR}(2x+1, 2k+1, l)=ii(x, k, \tfrac{1}{2}(l-2)),$$
$$\mathrm{RR}(2x, 2k, l)=ii(x, k, \tfrac{1}{2}l)+ii(x, k, \tfrac{1}{2}(l-1));$$

$$\mathrm{IR}(2x+1, 2k, l)=\tfrac{1}{2}\{ii(2x+1, 2k, l)-ii(x+1, k, \tfrac{1}{2}l)\},$$
$$\mathrm{IR}(2x+1, 2k+1, l)=\tfrac{1}{2}\{ii(2x+1, 2k+1, l)-ii(x, k, \tfrac{1}{2}(l-2))\},$$
$$\mathrm{IR}(2x, 2k, l)=\tfrac{1}{2}\{ii(2x, 2k, l)-ii(x, k, \tfrac{1}{2}l)-ii(x, k, \tfrac{1}{2}(l-1))\};$$

$$\mathrm{R}^2\mathrm{R}^2(4x+1, 4k, l)=ii(x+1, k, \tfrac{1}{4}l),$$
$$\mathrm{I}^2\mathrm{R}^2(4x+1, 4k, l)=\tfrac{1}{2}\{ii(2x+1, 2k, \tfrac{1}{2}l)-ii(x+1, k, \tfrac{1}{4}l)\},$$
$$\mathrm{RR}^2(4x+1, 4k, l)=ii(2x+1, 2k, \tfrac{1}{2}l)-ii(x+1, k, \tfrac{1}{4}l),$$
$$\mathrm{IR}^2(4x+1, 4k, l)=\tfrac{1}{4}[ii(4x+1, 4k, l)+2ii(x+1, k, \tfrac{1}{4}l)$$
$$-3ii(2x+1, 2k, \tfrac{1}{2}l)];$$

$$\mathrm{R}^3\mathrm{R}^3(6x+1, 6k, l)=ii(x+1, k, \tfrac{1}{6}l),\ \mathrm{R}^3\mathrm{R}^3(7, 3, 3)=1,$$
$$\mathrm{I}^3\mathrm{R}^3(6x+1, 6k, l)=\tfrac{1}{2}\{ii(2x+1, 2k, \tfrac{1}{3}l)-ii(x+1, k, \tfrac{1}{6}l)\},$$
$$\mathrm{RR}^3(6x+1, 6k, l)=ii(3x+1, 3k, \tfrac{1}{2}l)-ii(x+1, k, \tfrac{1}{6}l),\ \mathrm{RR}^3(7, 3, 1)=2,$$
$$\mathrm{IR}^3(6x+1, 6k, l)=\tfrac{1}{6}\{ii(6x+1, 6k, l)+3ii(x+1, k, \tfrac{1}{6}l)$$
$$-ii(2x+1, 2k, \tfrac{1}{3}l)-3ii(3x+1, 3k, \tfrac{1}{2}l)\},$$
$$\mathrm{IR}^3(7, 3, 2)=\mathrm{IR}^3(7, 3, 1)=\mathrm{IR}^3(7, 3, 0)=1;$$

$$\mathrm{I}^n\mathrm{R}^m(x+1, k, x-k)=0.$$

By the aid of the above, together with the following, equations, the $(x+k+l)$-edra having $k+l$ triangular faces, an $(x+k+l-1)$-gonal base and triedral summits, are successively found.

$$\mathrm{I}(x+k+l, k+l)=\Sigma\{\mathrm{I}(x+k')\,.\,\mathrm{II}(x, k', l')+\mathrm{I}^2(x, k')\,.\,\mathrm{II}^2(x, k', l')$$
$$+\mathrm{I}^3(x, k')\,.\,\mathrm{II}^3(x, k', l')+\mathrm{R}(x, k')\,.\,\mathrm{IR}(x, k', l')$$
$$+\mathrm{R}^2(x, k')\,.\,\mathrm{IR}^2(x, k', l')+\mathrm{R}^3(x, k')\,.\,\mathrm{IR}^3(x, k', l')\};\ \text{\&c.\&c.}$$

taken for all values of $k'+l'=k+l$.

Similar equations are to be formed for the remaining five subdivisions of $P(x+k+l,\ k+l)$.

Of the products under Σ, the first factors are found by the preceding part of the process, and the second are given by the equations above written as solutions of the problem. The factors will of course frequently be zeros. Finally, if $x'=x+k+l$,

$$P_{x+k+l}=P_{x'}=P(x',2)+P(x',3)+\ldots+P(x',\tfrac{1}{2}(x'-1)).$$

Thus, to give an example,

$$\begin{aligned}
P_{11}&=P(11,2)+P(11,3)+P(11,4)+P(11,5)\\
&=I(11,2)+I(11,3)+I(11,4)+(I(11,5)=0)\\
&\quad+I^2(11,2)+I^2(11,4)\\
&\quad+R(11,2)+R(11,3)+R(11,4)+R(11,5).
\end{aligned}$$

$$I(11,2)=I^2(9,2)\,.\,II^2(9,2,0)+I(9,2)\,.\,II(9,2,0);$$

$$I(11,3)=I(8,2)\,.\,II(8,2,1)+R(8,2)\,.\,IR(8,2,1)+I(8,3)\,.\,I(8,3,0);$$

$$\begin{aligned}
I(11,4)=I^2(7,2)\,.\,II^2(7,2,2)&+R(7,2)\,.\,IR(7,2,2)\\
&+R^3(7,3)\,.\,IR^3(7,3,1);
\end{aligned}$$

$$I^2(11,2)=I^2(9,2)\,.\,I^2I^2(9,2,0);$$

$$I^2(11,4)=I^2(7,2)\,.\,I^2I^2(7,2,2);$$

$$R(11,2)=R(9,2)\,.\,RR(9,2,0);$$

$$R(11,3)=R(8,2)\,.\,RR(8,2,1);$$

$$R(11,4)=R(7,2)\,.\,R(7,2,2)+R^3(7,3)\,.\,RR^3(7,3,1);$$

$$R(11,5)=R(6,2)\,.\,RR(6,2,3).$$

The result is

$$P_{11}=I_{11}+I^2_{11}+R_{11}=61+7+12=80.$$

XV. "Notes on British Foraminifera." By J. Gwyn Jeffreys, Esq., F.R.S. Received June 19, 1855.

Having, during a great many years, directed my attention to the recent Foraminifera which inhabit our own shores, I venture to offer a few observations on this curious group, as Dr. Carpenter, who has favoured the Society with an interesting and valuable memoir on the subject, seems not to have had many opportunities of studying the animals in the recent state.

Rather more than twenty years ago I communicated to the Linnean Society a paper on the subject, containing a diagnosis and figures of all the species. This paper was read and ordered to be printed in the Transactions of that Society; but it was withdrawn by me before publication, in consequence of my being dissatisfied with D'Orbigny's theory (which I had erroneously adopted), that the animals belonged to the Cephalopoda; and my subsequent observations were confirmed by the theory of Dujardin. I have since placed all my drawings and specimens at the disposal of Mr. Williamson of Manchester, who has given such a good earnest of what he can do in elucidating the natural history of this group, by his papers on *Lagena* and the Foraminiferous mud of the Levant.

The observations which I have made on many hundred recent and living specimens of various species, fully confirm Dr. Carpenter's view as to the simple and homogeneous nature of the animal. His idea of their reproduction by gemmation is also probably correct; although I cannot agree with him in considering the granules which are occasionally found in the cells as ova. These bodies I have frequently noticed, and especially in the *Lagenæ*; but they appeared to constitute the entire mass, and not merely a part of the animal. I am inclined to think they are only desiccated portions of the animal, separated from each other in consequence of the absence of any muscular or nervous structure. It may also be questionable if the term "ova" is rightly applicable to an animal which has no distinct organs of any kind. Possibly the fry may pass through a metamorphosis, as in the case of the Medusa.

Most of the Foraminifera are free, or only adhere by their pseudopodia to foreign substances. Such are the *Lagena* of Walker, *Nodosaria, Vorticialis* and *Textularia,* and the *Miliola* of Lamarck. The latter has some, although a very limited, power of locomotion; which is effected by exserting its pseudopodia to their full length, attaching itself by them to a piece of seaweed, and then contracting them like india-rubber, so as to draw the shell along with them. Some of the acephalous mollusks do the same by means of their byssus. This mode of progression is, however, exceedingly slow; and I have never seen, in the course of twenty-four hours, a longer journey than a quarter of an inch accomplished by a *Miliola,* so that, in comparison with it, a snail travels at a railroad pace.

Some are fixed or sessile, but not cemented at their base like the testaceous annelids. The only mode of attachment appears to be a thin film of sarcose. The *Lobatula* of Fleming, and the *Rosalia* and *Planorbulina* of D'Orbigny belong to this division.

Dr. Carpenter considers the Foraminifera to be phytophagous, in consequence of his having detected in some specimens, by the aid of the microscope, fragments of *Diatomaceæ* and other simple forms of vegetable life. But as I have dredged them alive at a depth of 108 fathoms (which is far below the Laminarian zone), and they are extremely abundant at from 40 to 70 fathoms, ten miles from land and beyond the range of any seaweed, it may be assumed without much difficulty, that many, if not most of them, are zoophagous, and prey on microscopic animals, perhaps even of a simpler form and structure than themselves. They are in their turn the food of mollusca, and appear to be especially relished by *Dentalium Entale*.

With respect to Dr. Carpenter's idea that they are allied to sponges, I may remark that *Polystomella crispa* (an elegant and not uncommon species) has its periphery set round at each segment with siliceous spicula, like the rowels of a spur. But as there is only one terminal cell, which is connected with all the others in the interior by one or more openings for the pseudopodia, the analogy is not complete, this being a solitary, and the sponge a compound or aggregate animal.

I believe the geographical range or distribution of species in this group to be regulated by the same laws as in the Mollusks and other marine animals. In the gulf of Genoa I have found (as might have been expected) species identical with those of our Hebridean coast, and *vice versâ*.

In common with Dr. Carpenter, I cannot help deploring the excessive multiplication of species in the present day, and I would include in this regret the unnecessary formation of genera. Another Linnæus is sadly wanted to correct this pernicious habit, both at home and abroad.

The group now under consideration exhibits a great tendency to variation of form, some of the combinations (especially in the case of *Marginulina*) being as complicated and various as a Chinese puzzle. It is, I believe, undeniable, that the variability of form is in an inverse ratio to the development of animals in the scale of Nature.

Having examined thousands (I may say myriads) of these elegant organisms, I am induced to suggest the following arrangement:—

1. *Lagena* (Walker) and *Entosolenia* (Williamson).

2. *Nodosaria* and *Marginulina* (D'Orb.), &c.

3. *Vorticialis* (D'Orb.), *Rotalia* (Lam.), *Lobatula* (Flem.), *Globigerina* (D'Orb.), &c.

4. *Textularia* (Defrance), *Uvigerina* (D'Orb.), &c.

5. *Miliola* (Lam.), *Biloculina* (D'Orb.), &c.

This division must, however, be modified by a more extended and cosmopolitan view of the subject, as I only profess to treat of the British species. To illustrate MacLeay's theory of a quinary and circular arrangement, the case may be put thus.

The first family is connected by the typical genus *Lagena* with the second, and by *Entosolina* with the fifth; the second is united with the third through *Marginulina*; the third with the fourth through *Globigerina*; and the fourth with the last through *Uvigerina*.

Whether these singular and little-known animals are Rhizopodes, or belong to the Amœba, remains yet to be satisfactorily made out.

London, June 18, 1855.

XVI. "Preliminary Research on the Magnetism developed in Iron Bars by Electrical Currents." By J. P. Joule, F.R.S. Received June 21, 1855.

The author had, many years ago, found that the magnetism developed by electro-magnetic coils in bars of upward of $\frac{1}{3}$rd of an inch

diameter, was nearly proportional to the strength of the current and the length of the wire, any alteration, within certain limits, of the diameter of a bar being attended with only trifling effects, so long as the point of saturation was not nearly approached. The Russian philosophers Lenz and Jacobi had, however, stated that the magnetism developed was, *cæteris paribus,* proportional to the diameter of the bar. The discrepancy between the above results is considered by the author to be owing rather to the different circumstances under which the experiments were tried than to any inaccuracies in the experiments themselves. Further, it appeared to him that in any case of induction by electric currents, careful distinction should be made between the several effects, which, compounded together, constitute the total magnetic action. Especially should a distinction be made between the magnetism existing under the inductive influence of the current and that permanently developed so as to remain after the electrical circuit is broken, and therefore the first efforts of the author were directed to ascertain the laws which regulate this permanent effect, or, as he thinks it may be conveniently termed, the *magnetic set.*

In his experiments the magnetism of any bar was ascertained, by placing it vertically with its lower end near a delicately suspended magnetic needle. This was a piece of sewing-needle $\frac{3}{16}$ths of an inch long, furnished with an index of fine drawn glass tube traversing over a graduated circle six inches in diameter. It was suspended by a filament of silk. The tangent of the deflection of the needle was found to be the exact measure of the attraction of a bar. In working with this instrument, it was found that the resistance of the air prevented the needle from swinging even once beyond the point of equilibrium to which it always arrived in less than ten seconds. This resistance of the air, so useful for bringing the needle rapidly to a state of rest, rendered it necessary to keep the entire instrument at a uniform temperature, for the slightest local application of heat produced currents of air within the glass case of sufficient strength to occasion considerable deflections. The circumstance points to the possibility of constructing a new and very sensitive thermometer which might be useful, particularly in experiments on the conduction of heat.

The method of experimenting consisted in observing,—1st. the magnetic attraction of any bar when a current circulated through

its spiral; 2nd. the attraction still subsisting after the circuit was broken; 3rd. the attraction of the other pole of the needle on the reversal of the current; and 4th. the attraction remaining after this reverse current was cut off. The sum of the 1st and 3rd observations gives the total change in the magnetism of a bar by the reversal of the current. The sum of the 2nd and 4th gives the total permanent change of magnetism, or the *magnetic set*.

The experiments were made with iron bars of the several diameters, $\frac{1}{25\cdot6}$, $\frac{1}{17\cdot2}$, $\frac{1}{8}$, $\frac{1}{4}$, $\frac{1}{2}$, and one inch, the length being in each case one yard; and also with iron bars $\frac{1}{8}$, $\frac{1}{4}$, $\frac{1}{2}$ and one inch diameter, of the length of two yards. In all the bars of small diameter up to $\frac{1}{4}$ of an inch, the magnetic set obtained by the use of feeble currents was found to be proportional to the square of the current employed in producing them. This law was found to subsist through a long series of electric intensities; but when the current was increased to a certain amount, the *set*, as observed in the bars of $\frac{1}{25\cdot6}$ and $\frac{1}{17\cdot2}$ of an inch diameter, increased in a much higher ratio, so as to vary, in some instances, with the 4th and 6th powers of the current. The point at which this phenomenon takes place is called the *magnetic breaking point*. A further increase of the current was attended with a rapid decrease of this ratio as the saturation of the bar was approached.

The total change of magnetic condition by reversal of the current, minus the magnetic set, is found to be nearly proportional to the intensity of the current.

Results of exactly similar character were obtained by the use of an electro-magnet, consisting of a bar of hard steel $\frac{1}{4}$ of an inch in diameter and $7\frac{3}{4}$ inches long.

In conclusion, the author points out the striking and instructive analogy which exists between the above phenomena and those of the set of materials as exhibited by Professor Hodgkinson, who, in his admirable researches, has proved that the *set*, or permanent change of figure, in any beam is proportional to the square of the pressure to which it has been exposed.

Communications were read also from the Astronomer Royal and Mr. Macquorn Rankine *.

* Notices of these will appear in the next Number.

June 21, 1855. (Continued.)

The LORD WROTTESLEY, President, in the Chair.

The following communications were read:—

XVII. "Discussion of the observed Deviations of the Compass in several Ships, Wood-built and Iron-built; with General Tables for facilitating the examination of Compass-deviations." By G. B. Airy, Esq., F.R.S., Astronomer Royal. Received June 2, 1855.

The author refers, in the first place, to his paper in the Philosophical Transactions for 1839, on the Disturbance of the Compass in Iron Ships, for a theory of the forces produced by the transient induced magnetism of iron. Using the term "polar-magnet-deviation" to express a deviation similar to that which would be produced by a magnetized steel bar partaking of the movements of the ship; and using the term "quadrantal deviation" for a deviation following the law of the sine of double the azimuth, and thus having, if "positive," the signs $+-+-$ in the four successive quadrants of azimuth, or, if "negative," the signs $-+-+$ in the four successive quadrants: then it appears that the deviation produced by the transient induced magnetism of a ship will consist of two parts; of which one will be a "polar-magnet-deviation," such as would be produced by a magnetized steel bar whose axis is parallel to the keel of the ship, and whose absolute intensity is proportional to the terrestrial vertical force at the place; and the other will be a "quadrantal deviation," which, in angular deviation, will be absolutely the same in all magnetic latitudes and with all magnitudes of terrestrial magnetic force, and will usually be "positive." Now combining these forces with the force of the "sub-permanent magnetism" of a ship, which in its nature is essentially similar to the magnetism of a steel bar, and would therefore, if

isolated, produce "polar-magnet-deviation;" and remarking that the combination of two polar-magnet-forces will produce a third polar-magnet-force, the resulting deviations of which will be "polar-magnet-deviations;" it is evident that the analysis of the observed deviations of the compass in any given instance will consist in resolving them into two systems, one of which follows the laws of "polar-magnet-deviation," and the other is a "quadrantal deviation."

The practical solution of this problem, without the assistance of tables, is troublesome. In order to diminish the difficulty, the author has prepared a Table of polar-magnet-deviations, for the whole circumference as regards azimuths, and for all values of "modulus" or proportion of the disturbing force to the earth's horizontal force, up to 0·8.

To discover the elements of "polar-magnet-deviation," that is, the neutral point and the modulus, in any given case, it is necessary so to combine the observations that the "quadrantal deviation" shall be eliminated. Simple rules are given for this; and the process of investigating the elements, with the assistance of the Tables, is illustrated by exhibiting the work from beginning to end, in an actual instance.

When the elements are found, the Tabular Polar-Magnet-Deviations are to be formed from those elements; and the excess of the Observed Deviations over these Tabular Deviations ought to consist simply of Quadrantal Deviation and Errors of Observations

From the neutral point and the modulus, with the absolute measure of the terrestrial horizontal force, it is easy to form the absolute measures of the apparent permanent forces of the ship, in the directions of "headward" and "starboard" respectively. The starboard force (on the assumption of general symmetry) can arise from nothing but subpermanent magnetism; the headward force will consist of subpermanent magnetism added algebraically to a multiple of the terrestrial vertical force, the multiplier being an unknown constant different for each different ship.

The process is then applied by the author to four wood-built sailing ships, two wood-built steamers, and five iron-built steamers, whose compass-deviations have been observed at twenty-nine stations in all. The results are as follows:—

1. In all cases, the principal part of the deviation follows the law of polar-magnet-deviation.

2. When the polar-magnet-deviation is computed accurately from the Table, and subtracted from the observed deviation, the residual quantity in all cases follows very closely the law of quadrantal deviation, leaving very little to be accounted for by errors of observation.

3. For each ship, the coefficient of quadrantal deviation is sensibly the same in all localities. The small deviations from exact equality cannot be referred to geographical position, and evidently depend on accidental changes in the distribution of the iron, especially of that which is near to the compass.

4. As the test of theory must reside in the comparison of residual quantities, and as it appears that these residual quantities obey with great exactness the law which theory assigns, it follows that the theory, "that the deviation may in all cases be represented by a combination of two deviations, of which one is a polar-magnet-deviation, and the other is a quadrantal deviation whose angular coefficient, for the same ship, is constant under all circumstances," is practically accurate.

5. Consequently in every case the deviation at any locality may be perfectly corrected; by the application of a steel magnet to neutralize the polar-magnet-force; and of a mass of soft iron on one side of the compass, and at the same level, to correct the quadrantal deviation (as the author had previously explained).

6. The mass of soft iron will not require to be changed, under any circumstances. It will depend on the variability or constancy of the subpermanent force to determine whether the steel magnet must be or must not be changed in different localities or after the lapse of time.

7. On forming the expressions for the absolute values of the headward and starboard polar-magnet-forces, it appears that in some ships the subpermanent magnetism is really (to sense) a permanent magnetism, but that in others there is a sensible change. In one instance the change was such that, supposing the deviations to be accurately corrected by magnets and soft iron in England, there would have been at the Cape of Good Hope an error whose maxi-

mum was nearly $5\frac{1}{2}°$; in all other cases the error would be smaller, and in some practically insensible.

8. The proper way of counteracting these changes evidently is, to readjust the magnets; and for this purpose the magnets should be so mounted as to admit of adjustment at any time. The re-adjustment can be effected in a very short time when a ship is in port, and can probably also be very easily effected at sea, in favourable weather, when a compass of reference can be carried high up one of the masts.

9. The author strongly condemns any system of navigating a ship by forming a table of compass-deviations from observations at one place, and using that table until observations have been obtained at some other place. It does not in the smallest degree guard against the effect of change in the ship's subpermanent magnetism; and it introduces errors which are purely gratuitous and unnecessary, and which are entirely avoided if the compass is corrected by magnets and soft iron. In elucidation of the amount of errors that may be introduced, if from any cause this system is carried to extremes, he remarks that in the instance of the 'Trident,' sailing from the Thames to Rio Janeiro, the table of compass-deviations formed in the Thames would have been so erroneous when the ship arrived at Rio Janeiro, that on one course the error would have been 6° or 7° in one direction, and on another course it would have been 8° or 9° in the opposite direction: yet during this voyage the ship's subpermanent magnetism had not changed at all; and if the compasses had been corrected in the Thames by magnets and soft iron, there would not have been an error of a single degree in any part of the voyage. In other cases, where there was a real change of subpermanent magnetism, the error would have been fully doubled by carrying on the original table of observed compass-deviations.

The communication is closed by the Table of Polar-Magnet-Deviations. It is a table of double entry, one of the arguments being the azimuth of the ship's head from the neutral point, the other argument being the modulus or proportion of the polar-magnet-force to the terrestrial horizontal force. The azimuth is expressed in points and decimals of a point, and is given for every $0^{p}\cdot1$ from $0^{p}\cdot0$ to $16^{p}\cdot0$; the second half of the circle being a repetition

of the first, but with sign changed. The modulus is given for every 0·01 from 0·00 to 0·80. The corresponding deviations are given in degrees and minutes. For each modulus there is also given the mean of all the deviations in the semicircumference, for that modulus; by use of which, in comparison with the mean in any given instance, the modulus in that instance is determined.

XVIII. "On Axes of Elasticity and Crystalline Forms." By W. J. Macquorn Rankine, C.E., F.R.SS. L. & E. &c. Received June 14, 1855.

An Axis of Elasticity is any direction with respect to which any kind of elastic force is symmetrical.

In this paper the deviation of a molecule of a solid from that condition as to volume and figure which it preserves when free from the action of external forces, is denoted by the word "*Strain*," and the corresponding effort of the molecule to recover its free volume and figure by the word "*Stress*."

In devising a nomenclature for quantities relating to the theory of elasticity, *strain* is denoted in composition by *θλίψις*, and *stress* by *τάσις*.

Every possible strain of a molecule, when referred to rectangular axes, may be resolved into six *elementary strains*; three elongations or linear compressions, and three distortions. Every possible stress, when referred to rectangular axes, may be resolved into six *elementary stresses*; three normal pressures, and three tangential pressures, which tend to diminish the corresponding elementary strains.

The elementary strains being in fact approximately linear functions of the elementary stresses, are treated in this investigation as exactly so.

The sum of the six integrals of the elementary stresses, each taken with respect to the corresponding elementary strain *from* its actual amount *to* zero, is the *Potential Energy of Elasticity*, and is a homogeneous function of the elementary strains of the second order, and of twenty-one terms, whose constant coefficients are here called the *Tasinomic Coefficients*, or coefficients of Elasticity.

The principles of the Calculus of Forms, and in particular the *Umbral Notation* of Mr. Sylvester, are applied to the Orthogonal Transformations of the Tasinomic Coefficients.

Several functions of these coefficients are determined, called *Tasinomic Invariants,* which are equal for all systems of orthogonal axes in the same solid.

Certain functions of the Tasinomic Coefficients constitute the coefficients of two *Tasinomic Ellipsoids,* designated respectively as the *Orthotatic* and *Heterotatic* Ellipsoids, whose axes have the following properties.

Orthotatic Axes.

At each point of an elastic solid there is one position in which a cubical molecule may be cut out, such, that a uniform dilatation or condensation of that molecule by equal elongations or compressions of its three axes, will produce no tangential stress at the faces of the molecule.

The existence of orthotatic axes in a solid constituted of mutually attracting and repelling physical points was first proved by Mr. Haughton; it is proved in this paper independently of any hypothesis as to molecular structure or action.

Heterotatic Axes.

At each point of an elastic solid there is one position in which a cubical molecule may be cut out, such, that if there be a distortion of that molecule round x (x *being any one of its axes) and an equal distortion round* y (y *being either of its other two axes), the normal stress on the faces normal to* x *arising from the distortion round* x, *will be equal to the tangential stress around* z *arising from the distortion round* y.

The six coefficients of the Heterotatic Ellipsoid represent parts of the elasticity of a solid which it is impossible to reduce to attractions and repulsions between points.

Fifteen constants called the *Homotatic Coefficients,* which are composed of Tasinomic Coefficients and their linear functions so constituted as to be independent of the *Heterotatic Coefficients,* are the coefficients of the fifteen terms of a homogeneous biquadratic function of the coordinates, which being equated to unity, characterizes the

Biquadratic Tasinomic Surface. This surface, for solids composed of physical points, was discovered by Mr. Haughton; it is here investigated independently of all hypothesis.

By rectangular linear transformations, three functions of the Homotatic Coefficients may be made to vanish. Three orthogonal axes are thus found, which are called the *Principal Metatatic Axes*, and have the following property: *if there be a linear elongation along any one of these axes, and an equal linear compression along any other, no tangential stress will result on planes normal to these two axes.*

In each of the three planes of the principal Metatatic Axes, there is a pair of Diagonal Metatatic Axes bisecting the right angles formed by the pair of principal axes in the same plane.

In each plane in an elastic solid, there is a system of two pairs of metatatic axes, making with each other eight equal angles of 45°.

Various kinds and degrees of symmetry are pointed out, which the tasinomic coefficients may have with respect to orthogonal axes.

The Potential Energy of Elasticity may be expressed as a homogeneous function of the second order of the Elementary Stresses. The twenty-one coefficients of this function are called Thlipsinomic Coefficients.

The Thlipsinomic and Tasinomic Coefficients are related to each other as Contragredient Systems.

The Orthogonal and Diagonal Tasinomic and Thlipsinomic Axes coincide.

For the complete determination of the properties of the Homotatic Coefficients, it is necessary to refer them to oblique axes of coordinates.

The application of oblique co-ordinates to this purpose is much facilitated by the employment along with them of three auxiliary variables called *Contra-ordinates.* The contra-ordinates of a point R are the projections of the radius-vector OR on the three axes. For rectangular axes, co-ordinates and contra-ordinates are identical. The co-ordinates x, y, z and contra-ordinates u, v, w of a point R are connected by the equation

$$ux+vy+wz=\overline{\mathrm{OR}}^2.$$

As there are six independent quantities in the directions of a system of three axes of indefinite obliquity, there is a system of

right or oblique axes in every solid for which six of the coefficients of the characteristic function of the Biquadratic surface disappear, reducing that function to its canonical form of nine terms. These three axes are called the

Principal Enthytatic Axes.

Every Enthytatic axis has this property, *that a direct linear elongation or compression along such an axis, produces a normal stress, and no oblique or tangential stress on a plane normal to the same axis.*

Every Enthytatic axis is normal to the Biquadratic Surface, and is a line along which the direct elasticity of the body is either a maximum or a minimum, or in that condition which combines the properties of maximum and minimum.

It is probable that the faces or edges of primitive crystalline forms are normal to Enthytatic axes, and that the planes of cleavage in crystals are normal to Enthytatic axes of minimum elasticity.

It is also probable that the symmetrical summits of crystals correspond to Enthytatic axes.

There are, in every solid, at least the three principal Enthytatic Axes, which are normal to the faces of a hexahedron, right or oblique as the case may be. In certain cases of symmetry of these axes and of the Homotatic Coefficients, there are *Secondary or Additional Enthytatic Axes*, which are determined by the following principles:

1. When the three principal axes and the Homotatic Coefficients are symmetrical round a central axis, that axis is an additional Enthytatic axis.

2. When there are a pair of orthogonal Enthytatic axes in a given plane, there may be, under certain conditions specified in the paper, a pair of *additional or secondary axes* in that plane, making with each other a pair of angles bisected by the orthogonal axes.

In the first column of a table, the possible systems of Enthytatic axes are arranged according to a classification and nomenclature of their degrees and kinds of symmetry; and in the second and third columns are stated the primitive crystalline forms to the faces and edges of which such systems of axes are respectively normal, and which embrace all the primitive forms known in crystallography.

The six Heterotatic Coefficients are independent of the fifteen Homotatic Coefficients which determine the Enthytatic axes.

The paper concludes with observations on some real and alleged differences between the laws of solid elasticity and those of the luminiferous force,—on some hypotheses in connexion with the wave-theory of light,—and on the refraction of light in crystals as connected with the symmetry of their Enthytatic axes.

"Report of a Committee appointed by the Council to examine the Calculating Machine of M. Scheutz." Inserted for the information of the Fellows by order of the President and Council*.

The various applications of mathematics to physical questions, or to the transactions of common life, continually require the computation of numerical results. At one time isolated results have to be calculated from particular formulæ; at another it is required to calculate a series of values of the same analytical formula; in other words, to tabulate a function. It is only in the latter case that different instances have so much in common as to permit of the application of general methods irrespective of the particular function to be calculated. But even in the tabulating of functions one or other of two objects may be kept in view. At one time a result may be arrived at expressed in a complicated, perhaps transcendental, formula, and the mathematician may desire to know merely the general progress of the function. In such a case it will be sufficient to calculate values at rather wide intervals, and the mode of calculation must depend upon the peculiar function. But at other times functions present themselves which are of such common occurrence, or of such practical importance, that it is desirable to tabulate them for values of the variable increasing by small steps. In these cases general methods of interpolation come into use: it is sufficient to perform the calculations directly for comparatively wide intervals of the variable, and the intervening values of the function can be supplied by the mere addition of differences.

* The Committee consisted of Prof. Stokes, Sec. R.S., Prof. W. H. Miller, Prof. Wheatstone, and the Rev. Prof. Willis.

It is well known that Mr. Babbage was the first person who conceived the idea of performing all these systems of additions mechanically, and thereby saving both the mental labour and the risk of error attending their calculation in the ordinary way. This idea was actually carried out, and resulted in the invention of his Difference Engine. The engine, so far as it has yet been executed, was constructed at the public expense, and is now deposited in the Museum of King's College, London. The part constructed contains 19 digits and 3 orders of differences; and as all the essential movements are comprised in this part, a more extended engine would consist merely of the same members oftener repeated, and would not involve any additional difficulty of construction. It was part of Mr. Babbage's original design that machinery for printing off the results calculated should be included in his engine, and some of the mechanism for this purpose was actually executed. The portion placed in King's College contains machinery for calculating only. It does not fall within the province of this report to do more than mention the Analytical Engine subsequently invented by Mr. Babbage, as the machine of M. Scheutz is a Difference Engine, and nothing more.

A full account of the principles and action of Mr. Babbage's Difference Engine, but without any details of its mechanism, was published in the 'Edinburgh Review' for April to July, 1834. It was, as we are informed, the perusal of this paper which induced M. Scheutz to set about the invention of modes of mechanically executing the necessary changes. The result was the completion of the present engine, which has now for some time been in the apartments of the Royal Society. In this machine M. Scheutz has followed the general ideas of Mr. Babbage in the distribution of digits and differences, and in particular in throwing back the differences at every alternate order one stage, from whence results the possibility of acting simultaneously on all the odd and on all the even differences, and thereby making the machine advance one stage by two addition-motions only; whereas otherwise as many separate addition-motions would have been necessary as there were orders of differences retained. But the mechanism by which the additions and carriages are effected in the machine of M. Scheutz is different from that of Mr. Babbage. The engine is also provided with

mechanism for printing, or rather for furnishing stereotype plates of the calculated results.

As M. Scheutz has taken out a patent for his engine, it will be unnecessary to give a detailed description of the machinery, which may be obtained in the specification, a copy of which has been presented to the Royal Society. It will be sufficient to give an idea of its general construction and extent with a view of estimating its powers.

The machine takes in the function to be tabulated and the first four orders of differences, each to fifteen digits. Of these only the first eight (in the case of the function itself) are printed, the others being reserved to guard against errors arising from decimal places left out.

The places of the digits are represented by fifteen vertical spindles, around which, but not usually connected with which, are placed horizontal wheels in five separate tiers. Each wheel has its circumference divided into ten equal parts, and is marked with the digits 0, 1, 2, 3, 4, 5, 6, 7, 8, 9. In the normal state of the machine the numbers on the wheels of the highest tier represent the function (u_x) to be tabulated, and those on the tiers below represent respectively Δu_{x-1}, $\Delta^2 u_{x-1}$, $\Delta^3 u_{x-2}$, and $\Delta^4 u_{x-2}$. In each case the digits by which these numbers are represented run from left to right, as in print. The mechanism is such, that by turning a handle continuously in one direction an indefinite succession of movements is produced which are alternately backwards and forwards. The effect of the forward motion is, that the numbers on the third and the fifth tiers (or as they may conveniently be called the Δ^2 and Δ^4 tiers) add themselves respectively to those on the tiers above, altering thereby the positions of the wheels of the Δ^1 and Δ^3 tiers, while the wheels of the Δ^2 and Δ^4 tiers remain at rest; and the backward motion does for the Δ^1 and Δ^3 tiers what the forward motion does for the Δ^2 and Δ^4 tiers. Thus the numbers on the several tiers will be as follows:—

At first	u_x	Δu_{x-1}	$\Delta^2 u_{x-1}$	$\Delta^3 u_{x-2}$	$\Delta^4 u_{x-2}$;
After the forward motion	u_x	Δu_x	$\Delta^2 u_{x-1}$	$\Delta^3 u_{x-1}$	$\Delta^4 u_{x-2}$;
After the complete motion ..	u_{x+1}	Δu_x	$\Delta^2 u_x$	$\Delta^3 u_{x-1}$	$\Delta^4 u_{x-1}$,

$\Delta^4 u_{x-1}$ in the last term being written instead of $\Delta^4 u_{x-2}$, which is

allowable, since the fourth differences are supposed to be constant. Hence the effect of the complete motion, consisting of one forward and one backward motion, is to make all the numbers advance one stage; and therefore by continuing to turn the handle the numbers u_{x+1}, u_{x+2}, u_{x+3} &c., will be calculated in succession. According as these numbers are calculated they are impressed, by the action of the machine itself, on a plate of lead, by means of steel punches, while a numerator at the same time impresses beside them the values of the argument x. These plates are afterwards taken out, and stamped on an easily fusible alloy just on the point of solidifying, and thus are obtained stereotype plates of the calculated results, fit for printing from.

In retaining a given number of decimals, it is usual to add one to the last figure if the first digit left out be 5 or a higher number. This is effected in the machine in the simplest possible manner, namely by placing the cog which occasions the carriages from the ninth to the eighth place in the highest tier in such a position that the carriage takes place when the ninth wheel changes from 4 to 5, instead of from 9 to 0.

The principle of the machine is not of course dependent upon the circumstance that the radix of the scale of notation commonly employed has the particular value 10; and it would be as easy to construct a machine adapted to the senary or duodenary as to the denary scale. Not only so, but the machine actually constructed admits of being changed very readily from the denary to the senary scale, or rather to a mixture of the denary and senary scales, which is required in tabulating degrees, minutes, and seconds. For this purpose it is sufficient to take off the ordinary figure-wheels from those spindles which are to count by sixes, and put on spare wheels which are provided, adapted to the senary scale.

The machine works with the greatest freedom and smoothness. The parts move with the utmost facility, in fact, quite loosely. On this account no amount of dust which it would reasonably be expected to receive in any moderate time seems likely to interfere with its action. Besides, it can easily be taken to pieces and examined, if need be. Those motions which are not the direct consequences of the revolution of the handle acting through a train of rigid bodies are performed in consequence of gravity, no springs

being employed in the whole construction except two, the office of which is quite subordinate. When the parts are moved, they remain in their new places either from their weight or from friction, there being nothing to disturb them. This circumstance, which renders a wilful derangement of the machine exceedingly easy, permits of great simplicity and consequent cheapness of construction; nor does the machine seem likely to get out of order if reasonable care be taken of it.

The machine is competent to tabulate to any extent a function whose fourth differences are constant, so long as the expression of the numerical value of the function does not involve more than eight digits. The most general form of such a function is of course

$$a+bx+cx^2+dx^3. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Were the machine restricted to such functions, its use would be limited indeed; its utility must of course depend on its being applicable to functions in general, which, except in singular cases, may be expressed within a limited range of values of the variable x by a function of the above form. To estimate the capacities of the machine, or rather of a difference engine in general, whatever may be its particular construction, it will be necessary to investigate how soon the quantities neglected begin to tell in the result.

Now these quantities are of two kinds; first, the fifth and higher differences; secondly, the decimals of the fifteenth place. The effect of these may be examined separately. We may always suppose the first spindle to represent the first place of decimals, since it will only be necessary to multiply or divide by some power of 10 should that not be the case.

Suppose the machine set for u_x, and its first four differences (or to speak more exactly, the differences Δu_{x-1}, $\Delta^2 u_{x-1}$, $\Delta^3 u_{x-2}$, $\Delta^4 u_{x-2}$), and worked n periods, so as to give what ought to be u_{x+n}. We have

$$u_{x+n}=u_x+\frac{n}{1}\Delta u_x+\frac{n\,.\,\overline{n-1}}{1\,.\,2}\Delta^2 u_x+\ldots. \quad . \quad . \quad . \quad (2);$$

and since the machine would give u_{x+n} exactly if the fourth differences were constant, the error (E) will be

$$\frac{n\,.\,\overline{n-1}\,.\,\overline{n-2}\,.\,\overline{n-3}\,.\,\overline{n-4}}{1\,.\,2\,.\,3\,.\,4\,.\,5}\Delta^5 u_x+\frac{n\,.\,\overline{n-1}\,.\,\overline{n-2}\,.\,\overline{n-3}\,.\,\overline{n-4}\,.\,\overline{n-5}}{1\,.\,2\,.\,3\,.\,4\,.\,5\,.\,6}\Delta^6 u_x+\ldots.*$$

* This expression will not be absolutely exact, since it is Δu_{x-1}, $\Delta^2 u_{x-1}$,

The first term in this expression will usually be the most important; and for practical purposes the expression may be still further simplified. If n be tolerably large, the product $n\,.\,\overline{n-1}\,.\,\overline{n-2}\,.\,\overline{n-3}\,.\,\overline{n-4}$ may be replaced without material error by the fifth power of the arithmetic mean of the factors, or by $(n-2)^5$. Again, if y be the variable of which u is a function, x being merely the numeral marking the number of increments of y, each equal to k, we shall have near enough

$$\Delta^5 u_x = k^5 \frac{d^5 u}{dy^5},$$

so that

$$E = \frac{1}{120}(n-2)^5 k^5 \frac{d^5 u}{dy^5}.$$

In expressing a number to eight decimal places, we are always liable to an error which may amount to 5 in the ninth place. Hence $10^{-9}\times 5$ may be regarded as the greatest allowable error, though in truth the error should not be allowed to amount to this, if we wish to have the last figure true to the nearest decimal. Equating then E to $10^{-9}\times 5$, we find

$$n = 2 + \left(\frac{\cdot 0000006}{\frac{d^5 u}{dx^5}}\right)^{\frac{1}{5}} \cdot \frac{1}{k}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which gives the greatest number n of times the machine may be worked without stopping and fresh setting, so far as the limitation depends on the cause of error now under consideration. The increment of y during the action of the machine, which is equal to nk, or to $(n-2)k$ nearly, n being large compared with 2, is therefore nearly independent of the closeness or wideness of the intervals for which the value of the function is required, a given range, so to speak, of the function being taken in. Hence, so far as this cause of limitation is concerned, the utility of the machine will be proportional to the closeness of the intervals for which it is desired to tabulate the function.

Let us now consider the effect of the decimals omitted, retaining

$\Delta^3 u_{x-2}$, $\Delta^4 u_{x-2}$, and not Δu_x, $\Delta^2 u_x$, $\Delta^3 u_x$, $\Delta^4 u_x$ that are given correctly; but the inaccuracy thus arising in the estimation of the error committed by leaving out the fifth, &c. differences will plainly be insignificant.

only four orders of differences, since the effect of omitting the fifth and higher orders has been already investigated. Let E_1, E_2, E_3, E_4 be the errors left in the first, second, third, and fourth differences in setting the machine. Then in the same manner as before these may without sensible error be regarded as the errors in Δu_x, $\Delta^2 u_x$, $\Delta^3 u_x$, $\Delta^4 u_x$, although they are really the errors in Δu_{x-1}, &c., and we shall have for the error (E) in u_{x+n}

$$E=\frac{n}{1}\,\mathrm{E}_1+\frac{n\,\overline{n-1}}{1.2}\,\mathrm{E}_2+\frac{n.\overline{n-1}.\overline{n-2}}{1.2.3}\,\mathrm{E}_3+\frac{n.\overline{n-1}.\overline{n-2}.\overline{n-3}}{1.2.3.4}\,\mathrm{E}_4;$$

or, replacing the products as before,

$$E=n\mathrm{E}_1+\frac{1}{2}\left(n-\frac{1}{2}\right)^2\mathrm{E}_2+\frac{1}{6}(n-1)^3\mathrm{E}_3+\frac{1}{24}\left(n-\frac{3}{2}\right)^4\mathrm{E}_4. \quad . \quad . \quad (4)$$

If each of the quantities E_1, E_2, E_3, E_4 be liable to be as great as $10^{-16}\times 5$, the last term in this expression will be the most important if n be considerably greater than 4. Equating this term to $10^{-9}\times 5$, the greatest allowable error in E, we find

$$n-\frac{3}{2}=(24\times 10^7)^{\frac{1}{4}}, \qquad n=126 \text{ nearly},$$

so that the machine may be worked about 100 times without fresh setting.

In practice the limitation may be even less than this; for it may happen that $\Delta^4 u_x$ is smaller, perhaps much smaller, than $10^{-16}\times 5$, in which case the limitation will depend upon the absolute value of $\Delta^4 u_x$ or the possible value $10^{-16}\times 5$ of E_3, as the case may be. Should the restriction arise from the latter cause, we get by equating the third term in the second member of (4) to $10^{-9}\times 5$, $n=392$ nearly.

To illustrate these limitations by an example, suppose that it was required to make a table of sines to every minute. In this case we have

$$u=\sin y, \quad k=\frac{\pi}{180\times 60}=\cdot 0002909, \quad \frac{d^5u}{dy^5}=\cos y.$$

Putting for this last differential coefficient its greatest value unity, and substituting in (3), we get $n=196$ nearly. The fourth difference is very nearly equal to $-k^4\sin y$, which may contain figures in the fifteenth place, so that $n=126$ is about the greatest allowable value of n in consequence of the restriction arising from decimals

left out, which in this example is what limits the working. Should the intervals be a good deal wider than $1'$, as $5'$, it would then be the omission of fifth differences that would impose the limit, for the greatest allowable range on this account would be nearly the same as before, or about 3°, which would contain only thirty-six values to be calculated. Should it happen that both causes of error were about equally restrictive, it must be remembered that the corresponding errors in u_x would be comparable with one another, and might be added together; and in this case it may easily be shown that $126 \times 2^{-\frac{1}{4}}$, or 106 nearly, is somewhat inferior to the greatest allowable value of n. Should eight figures not be required to be retained, but seven, six, or five be sufficient, the last one, two, or three of the first eight spindles might be used for calculating instead of printing; and since the greatest allowable value of n, so far as depends on omission of decimals, varies nearly as the fourth root of the greatest allowable error in u_x, that value would be increased in the ratio of 1 to the fourth root of 10, or 100, or 1000, and from 126 would become 224, or 398, or 708. The greatest allowable value of n as regards the omission of fifth differences would increase in a somewhat slower ratio, since it varies nearly as the fifth root of the greatest allowable error in u_x. If, for example, it were 196, it would become 311, or 492, or 780.

The above is a fair specimen of the application of the machine. The particular function chosen is, it is true, a familiar one, which has been long since tabulated, but it is not the worse fitted for an example on that account. It may be seen at once how much mental labour and risk of error is saved by the use of such a machine, when tables have to be calculated to close intervals. The whole exertion of mind is confined to calculating the function and its differences at wide intervals, say for every 100th or 60th number to be tabulated, and setting the machine. Even this exertion (except so far as relates to the setting, which is easy,) might be reduced to one half, if desired, by setting the machine to calculate backwards as well as forwards. In order to give in succession the numbers $u_x, u_{x+1}, u_{x+2}, \ldots$ the machine has to be set to

$$u_x \quad \Delta u_{x-1} \quad \Delta^2 u_{x-1} \quad \Delta^3 u_{x-2} \quad \Delta^4 u_{x-2},$$

or to

$$u_x \quad \Delta D^{-1} u_x \quad \Delta^2 D^{-1} u_x \quad \Delta^3 D^{-2} u_x \quad \Delta^4 D^{-2} u_x,$$

D denoting as usual the operation $1+\Delta$. In order to give in succession the numbers u_x, u_{x-1}, u_{x-2}, ... the machine would simply have to be set to

$$u_x \quad \Delta' D'^{-1} u_x \quad \Delta'^2 D'^{-1} u_x \quad \Delta'^3 D'^{-2} u_x \quad \Delta'^4 D'^{-2} u_x,$$

if $D' u_x$ be used to denote u_{x-1}, and Δ' to denote $D'-1$. But $D' = D^{-1}$, and $\Delta' = D'-1 = D^{-1}-1 = -D^{-1}\Delta$, so that the required numbers are

$$u_x \quad -\Delta u_x \quad \Delta^2 D^{-1} u_x \quad -\Delta^3 D^{-1} u_x \quad \Delta^4 D^{-2} u_x,$$

or

$$u_x \quad -\Delta u_x \quad \Delta^2 u_{x-1} \quad -\Delta^3 u_{x-1} \quad \Delta^4 u_{x-2}.$$

Hence the numbers on the top, Δ^2, and Δ^4 tiers are the same as for the forward calculation, while those on the Δ and Δ^3 tiers are the arithmetical complements of the numbers found on those tiers after the machine has made one complete movement in calculating forwards from u_x. The printing part, however, is not adapted to such a change: the numbers would be printed off correctly, but in a wrong order; so that unless some reversing movement were introduced into the printing part, the printed results would only serve to set types from.

In the example chosen above, and in similar cases, the differences required for setting the machine would be calculated from their mathematical expressions. It might, however, be required to tabulate for small intervals a function which had been given by observation for larger ones, or to tabulate a mathematical function of so complicated a form that the differences could not be got directly without great trouble. In such a case there would be no difficulty; the differences for the smaller intervals would first have to be calculated from those for the larger ones by formulæ in finite differences, and then the setting and working of the machine would proceed as before.

It must be confessed, however, that except in the case of mathematical tables like those of sines, cosines, logarithms, &c., it is not ordinarily required to tabulate functions to intervals at all approaching, in closeness, to those in the example selected. Hence it is mainly, as it seems to us, in the computation of mathematical tables that the machine of M. Scheutz would come into use. The most

important of such tables have long since been calculated; but various others could be suggested which it might be worth while to construct, could it be done with such ease and cheapness as would be afforded by the use of the machine. It has been suggested to us too, and we think with good reason, that the machine would be very useful even for the mere reprinting of old tables, because it could calculate and print more quickly than a good compositor could set the types, and that without risk of error.

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W. H. Miller.
C. Wheatstone.
R. Willis.

P.S. Some time since, I received from Mr. Babbage, to whom I had written for information on one point connected with his machine, a letter, written subsequently to his first answer, in which he said that he had forgotten to mention an addition to his machine which enabled it to calculate a function when the last differences, instead of being constant, were dependent on the functions then under calculation in the other parts of the machine, provided the coefficients of the variable part were small enough to be expressed by a moderate number of digits. This was especially designed for the calculation of astronomical tables, where a difficulty occurs in the application of a machine with constant differences, arising from the circumstance that in the case of functions of short period the omitted differences soon become sensible even though the coefficients be but small. Mr. Babbage did not then recollect that this contrivance was accessible to the public, but in a subsequent letter he pointed out that such was the case. The following is an extract from this letter :—

"1st. The portion at Somerset House contains axes specially prepared for what (at this instant) I recollect to have familiarly called 'eating its own tail.'

"2nd. The drawings contain the modes of governing those axes in the finished engine.

"These are public property, and open daily to public inspection, which I suppose must be considered as publication. On referring to the 9th Bridgewater Treatise, second edition, I find (p. 34)

that I have used as an illustration a series computed by that very machine. * * *."

In the same letter Mr. Babbage refers to the following documents:—

Extract from a letter of Mr. Babbage to Sir H. Davy, 3 July, 1822, printed by order of the House of Commons. No. 370, 1823:—

" Another machine, whose plans are more advanced than several of those just named, is one for constructing tables which have no order of differences constant (p. 2).

" I should be unwilling to terminate this letter without noticing another class of tables of the greatest importance, almost the whole of which are capable of being calculated by the method of differences. I refer to all astronomical tables for calculating the places of the sun and planets. It is scarcely necessary to observe that the constituent parts of these are of the form $a \sin \theta$." (p. 5.)

He refers also to an extract from the Address of H. T. Colbroke, Esq., President of the Astronomical Society, on presenting to him the first medal given by the Society, 1824; and to a description of his machine by the late Mr. Baily, published in Schumacher's 'Astronomische Nachrichten,' No. 46, and republished in the 'Philosophical Magazine' for May 1824, p. 355. This last paper describes fully what could be done by the new contrivance.

I have ventured to insert this postscript without consulting my colleagues, as it is desirable not to delay the publication.

G. G. Stokes.

London, Oct. 5, 1855.

" Report made to the President and Council of the Royal Society, of Experiments on the Friction of Discs revolving in Water." By James Thomson, Esq., C.E., Belfast.

[A Committee of the British Association for the Advancement of Science, consisting of James Thomson, Esq., C.E., and William Fairbairn, Esq., C.E., F.R.S., having been appointed "to make Experiments on the Friction of Discs revolving in water, with especial reference to supplying data wanted in calculations relative to

the action and efficiency of Turbine Water-Wheels in general, and of Centrifugal Pumps; and also to make an experimental investigation relative to the action and efficiency of Centrifugal Pumps in general, and the amount of improvement derivable in them by the employment of an exterior whirlpool;" a sum of £50 from the Government Grant of 1853 was allotted by the Council of the Royal Society in aid of the inquiry. The experiments, as originally contemplated, have been arranged and conducted by Mr. Thomson, and the present Report of his progress is here inserted by order of the President and Council for the information of the Fellows.]

In last year's Report of the Committee it was stated, that an apparatus for making experiments on the friction of discs revolving in water had been constructed, and that experiments had been commenced with it. I have now further to state respecting the experiments for which that apparatus was adapted, that I have since got them completed and carefully arranged for the purpose of obtaining from them laws applicable for practical use.

I now beg to lay before the Royal Society, as a brief statement of the most essential results, the following general equation to show the relation between the velocity of revolution of the disc, the diameter of the disc, and the mechanical work consumed in friction:—

$$z=\frac{y^3d^5}{90,000},$$

in which d=diameter of the disc in feet,

y=number of revolutions of the disc per minute,

and z=number of foot-pounds of mechanical work consumed per minute.

This equation is based on experiments which range for the most part between the limits $yd=192$ and $yd=518$, and may be used with confidence, as sufficiently correct for most practical purposes, if the product of the number of revolutions per minute and the diameter of the disc in feet be between those limits. It is to be observed that the friction is slightly affected by the width of the water space within the case, and the coefficient 90,000 stated in the formula above is, for simplicity in the present brief report, taken between the coefficients obtained by two sets of experiments with different widths. A full report on the experiments already made,

explaining the manner of conducting them and stating the detailed results, would be rather lengthened, and would require drawings and diagrams, for all of which I have carefully preserved the requisite data; but before proceeding to put these in form suitable to be submitted to the Royal Society, I am desirous of prosecuting the remainder of the very interesting and important experiments which have been entrusted to me,—that portion of the whole, namely, which relates especially to centrifugal pumps. I have also to state, that if my engagements permit, I should be desirous of proceeding with a renewed and more extended set of experiments on the friction of discs, with an apparatus depending on the same leading principle as that which I have already used,—a principle which on trial has been found remarkably well suited for the desired purpose. For the attainment of greater accuracy and of a wider range of the experiments, it seems to me that no better method of procedure could be adopted, than to follow the same leading principles, with an apparatus of rather more refined construction, involving such improvements in details as have been suggested by the experience gained in the course of the experiments already made, and for the sake of greater steadiness of motion, worked by steam power instead of the hand of an operator. Should I have it in my power to conduct this renewed set of experiments, a detailed account of them will be preferable to a detailed account of those already made.

In respect to the experiments on Centrifugal Pumps, I have to say that I have prepared plans for an experimental apparatus on principles which I consider are peculiarly well suited for the attainment of useful and accurate results, and that I intend to proceed with the experiments as soon as my engagements shall permit.

I have further to state, that from the Experimental Fund of £50 granted by the Royal Society, the entire outlay as yet incurred has been £6 5*s.* 9*d*, leaving a balance of £43 14*s.* 3*d.* for the more extended experiments yet remaining to be made.

JAMES THOMSON.

Belfast, April 13, 1855.

The following account of the appropriation of the sum of £5000, placed by Her Majesty's Government, in five annual sums of £1000 each, in the years 1850 to 1854 inclusive, at the disposal of the Royal Society, to be employed in aiding the promotion of Science in the United Kingdom, has been presented to the Treasury, and is here printed for the information of the Fellows, by direction of the President and Council.

A. *Appropriation of the Government Grant of* £1000 *in the year* 1850.

1. For the publication of the Observations made at the Armagh (private) Observatory for the re-observation of Bradley's Stars; the work so published to be the property of Her Majesty's Government £350

The printing of this work is still in progress, and will shortly be completed.

2. For the publication of Vol. I. of the Catalogue of Ecliptic Stars observed at the Markree (private) Observatory; the work so published to be the property of Her Majesty's Government 150

This work has been printed; and the greater part of an impression of 500 copies presented, in the name of the British Government, to public institutions at home and abroad, and to individuals cultivating Astronomy in this and other countries, under the direction of a committee consisting of Sir John Herschel, the Astronomer Royal, and the Superintendent of the Nautical Almanack. The remainder of the impression is on sale at a low price; the proceeds to be credited to Her Majesty's Government.

3. To Charles Brooke, Esq., to be employed in the construction of an instrument for the autographic registry of the variations of the terrestrial magnetic force corrected for temperature 100

This instrument was completed and exhibited in the Great Industrial Exhibition for 1851.

4. To T. Wharton Jones, Esq., to be employed in assisting him in investigations on inflammation £100

These investigations were in continuation of an inquiry in which Mr. Jones had been for some time engaged, and for which he had obtained, in 1850, the Triennial Prize of £300, founded by Sir Astley Cooper. Their continuation has led to further publications in the Philosophical Transactions and in the Medico-Chirurgical Transactions, and is still in progress.

5. To Professor Owen, to have drawings made of undescribed or unfigured parts of the skeleton of the Megatherium. 100

The drawings have been made and are deposited at the Royal Society, accompanied by a memoir drawn up by Professor Owen, part of which has been published in the Philosophical Transactions, and the remainder is now in course of publication.

6. To Lieut.-Col. Sabine, for the purchase of magnetical and meteorological instruments of a new construction for trial of their merits at the Kew Observatory. The instruments to be the property of Her Majesty's Government 100

These instruments were purchased and their merits examined at the Kew Observatory: the results of the examination have been published in the Transactions of the British Association.

7. To Dr. Stenhouse, to assist his researches into the chemical relations subsisting amongst the various genera of plants 100

This grant produced a valuable paper "On the Action of Nitric Acid on various Vegetables," which was published in the Philosophical Transactions; and the full amount of the grant was subsequently placed again by Dr. Stenhouse at the disposal of the Committee, and became the subject of a fresh appropriation in 1853.

Total sum appropriated in 1850.... £1000

B. *Appropriation of the Government Grant of* £1000 *in* 1851.

1. To Dr. Thurnam, to assist in procuring exact drawings of crania of early British races £50

The drawings have been made, and are deposited with an accompanying memoir at the Royal Society.

2. To Professor Stokes, for experiments to determine the index of friction in different gases 175

These experiments are in progress at the Kew Observatory under Professor Stokes's direction.

3. To Dr. Hofmann, for a continuation of his investigations respecting organic bases 100

These investigations were in continuation of researches of which the results were published in the Philosophical Transactions, and for which a Royal Medal was awarded. They are regarded by chemists as extremely important, and are still in progress.

4. To the Astronomer Royal, for the reduction and publication of the late Rev. T. Catton's Astronomical Observations. 50

These observations, extending from 1791 to 1832, have been reduced, and the results published in the Transactions of the Royal Astronomical Society.

5. To John F. Miller, Esq. of Whitehaven, to obtain observations on the fall of rain, and on the minimum temperature in winter, at several stations in the Lake District of England. 50

These observations have been communicated to the Royal Society, and the results have been published in "Reports on the Meteorology of the Lake District," by Mr. Miller.

6. To Dr. W. B. Carpenter, for the execution of drawings of Foraminifera collected on the Australian coast during the Surveying Expedition of Her Majesty's Ship 'Fly' 25

These drawings have been deposited at the Royal Society, and have served, together with similar drawings procured by grants in 1852 and 1854, as data from which Dr. Carpenter has drawn up an important mono-

graph on this class of animals, which will be published in the forthcoming volume of the Philosophical Transactions.

7. To Leonard Horner, Esq., for the analysis of specimens of the water of the Nile, and of the soil at different depths in the valley of the Nile, which had been procured by the aid of a grant from the Donation Fund of the Royal Society .. £50

The results of this analysis have been published in the Philosophical Transactions.

8. To William Hopkins, Esq. of Cambridge, for investigations on the effect of pressure on the temperature of fusion of certain substances.............................. 250

These important experiments, in which Mr. Hopkins has been assisted by Messrs. Fairbairn and Joule of Manchester, are still in progress. A report of the results hitherto obtained is expected to be presented to the Royal Society at its next session.

9. To Dr. Miller, Mr. Gassiot, and Colonel Sabine, representing the Kew Committee of the British Association, for the construction and verification of standard meteorological instruments 150

By the aid of this and of a subsequent grant of equal amount in 1852, the Kew Committee have been enabled to meet satisfactorily the extensive applications which have been made to them by the Governments of our own country and of the United States, to provide and verify meteorological instruments required for the marine meteorological researches undertaken by those governments with a view to the interests of trade and navigation, as well as to those of general science.

10. To Professor Owen, to defray the cost of drawings of undescribed and unfigured fossils from South America and Australia 100

These drawings have been executed, and are seventy-two in number.

Total sum appropriated in 1851.... £1000

C. *Appropriation of the Government Grant of* £1000 *in* 1852.

1. To Professor William Thomson of Glasgow, for experimental researches in several branches of electrical science..	£50

This grant, as well as a subsequent one of £50 in 1853, was designed to assist in furnishing apparatus for various electrical researches in which Professor Thomson was engaged; the results, as far as they have yet been obtained, have been communicated to the Royal Societies of London and of Edinburgh, and published in their Transactions and in other scientific journals. The researches are still in progress.

2. To Dr. Tyndall, for experimental researches on the diathermic and conductive capacities of crystalline and other bodies....................................	50

This money has been expended in providing apparatus for the experimental researches referred to. The results, so far as they have been yet obtained, have been published in papers in the Philosophical Transactions, and the researches are still in progress.

3. To Professor Williamson, for experimental investigations into the law of the chemical action of masses..........	100

The experiments are in progress, and the results hitherto obtained will shortly be communicated to the Royal Society.

4. To Mr. Joule of Manchester, for experiments on the effects of magnetism on the dimensions of iron and steel bars.	30

These experiments are still in progress.

5. To Mr. John A. Dale, of Balliol College, Oxford, for experiments on the relation of metals with each other, and with liquids in the voltaic circuit......................	50

These experiments are still in progress.

6. To Professor Owen, for obtaining anatomical drawings of undescribed existing and extinct animals	100

Drawings, forty-three in number, have been executed, and part of the grant yet remains to be similarly applied.

7. To Dr. Miller, Mr. Gassiot, and Col. Sabine, for the construction and verification of standard meteorological instruments £150

See note to a similar appropriation in 1851, No. 9.

8. To Henry Gray, Esq., for investigations concerning the spleen 100

The results of these investigations are published in an Essay on the Spleen, for which the triennial Astley Cooper Prize of £300 was awarded in 1853.

9. To Professor Beale, for investigations into the chemistry of morbid products.. 50

This investigation is still in progress.

10. To Dr. Carpenter, for the execution of drawings of Foraminifera 25

See note to a similar appropriation in 1851, No. 6.

11. For the publication of Vol. II. of the Markree Catalogue of Ecliptic Stars 130

Vol. II. has been published, and the edition of 500 copies disposed of as in the case of Vol. I. (1850, No. 2.)

12. To Professor William Thomson and Mr. Joule, for experiments on the thermal effects experienced by fluids in passing through small apertures 100

The experiments referred to in this and in subsequent grants of £100 in 1853, and of £200 in 1854, are still in progress. A memoir describing part of the results obtained, has been presented to the Royal Society, and printed in the Philosophical Transactions.

13. To Dr. Frankland, for investigations into organo-metallic compounds 65

The results of this investigation have been communicated to the Royal Society in a memoir which will appear in the forthcoming volume of the Philosophical Transactions.

Total sum appropriated in 1852.... £1000

D. *Appropriation of the Government Grant of* £1000 *in* 1853.

1. To Dr. Waller, for investigating the results of the section of nerves.......... £100

An interim report of the progress of this investigation has been received by the Royal Society.

2. To Dr. James Thomson, C.E. of Glasgow, and Mr. Fairbairn of Manchester, for experiments on the friction of discs revolving in water, for the purpose of obtaining data required in calculations relating to turbine water-wheels and centrifugal pumps.......... 50

The results already obtained have been communicated to the Royal Society, and preparations have been made for renewing the experiments on a more extended scale.

3. To Captain Lefroy, for the expenses of preparing for publication observations on the Aurora Borealis made in North America.......... 20

The observations have been in great measure prepared for publication.

4. To Warren De la Rue, Esq., for mounting the Huygenian object-glass of 123 feet focal length.......... 250

This, with an appropriation of equal amount in 1854, was designed to meet an application made to the Royal Society by M. Struve of St. Petersburg, to compare the appearance of Saturn as shown by the Huygenian lens referred to with that of the planet as seen in telescopes of modern date; in consequence of Huygens's representation of the ring not according with its appearance as now observed. Difficulties have been met with in carrying out this object in the method first proposed, which have occasioned delay; and the subject now stands for reconsideration.

5. To Professor William Thomson of Glasgow, for experiments on the thermal effects of electric currents in unequally heated conductors.......... 50

See note to a similar appropriation in 1852, No. 1.

6. To Professor William Thomson and Mr. Joule, for continuing the experiments on the thermal effects experienced by fluids in passing through small apertures £100 0 0

See note to a similar appropriation in 1852, No. 12.

7. To Dr. Marcet, for expenses connected with his researches on the excretions of man and animals. 50 0 0

The results were communicated in a paper presented to the Royal Society, and printed in the Philosophical Transactions.

	Total sum appropriated in 1853. . . .	£620 0 0
	Balance to credit in 1854	480 10 11
Grant from Government	£1000 0 0	
Replaced by Dr. Stenhouse	100 0 0	
Repaid from the Catton grant	0 7 0	
Repaid from the Ecliptic Stars' grant	0 3 11	
	£1100 10 11	£1100 10 11

E. *Appropriation of the Government Grant of* £1000 *in* 1854, *and of a balance carried from the preceding year of* £480 10*s.* 11*d.*

1. To Robert Mallet, Esq., C.E., Dublin, for experiments on earthquake waves. £150

The apparatus for these experiments was prepared by means of a grant from the British Association. The experiments are proposed to be made at Holyhead, when a fitting time is arrived in the progress of the harbour works at that station.

2. To Dr. Marcet, for a continuation of his researches on the excretions of man and animals 50

See note to a similar appropriation in 1853, No. 7.

3. To Professor Eaton Hodgkinson, for experiments on the strength of materials 100

This appropriation has been augmented by a gift of £200 from Robert Stephenson, Esq., C.E. The experiments are in progress.

4. To Dr. Tyndall, for experimental researches in heat and magnetism £100

The results of a part of these researches have been presented to the Royal Society, and will be published in the forthcoming volume of the Philosophical Transactions.

5. To Dr. Woods of Parsonstown in Ireland, for experimental researches on the heat developed in the oxidation of certain metals 20

The experiments are in progress, and an interim report has been presented to the Royal Society.

6. To Professor William Thomson of Glasgow and Mr. Joule of Manchester, for experimental researches on fluids in motion, and on the thermal effects experienced by fluids in passing through small apertures 200

See note to an appropriation for the same purpose in 1852, No. 12, and in 1853, No. 6. A memoir containing the results of these researches, so far as they have yet been completed, has been presented to the Royal Society, and printed in the Philosophical Transactions.

7. To Warren De la Rue, Esq., for mounting the Huygenian object-glass 250

See note to a similar appropriation in 1853, No. 4.

8. To T. H. Huxley, Esq., for the publication of his zoological researches.................................... 300

Mr. Huxley was employed under the orders of the Admiralty in a Surveying Expedition under Captain Owen Stanley, during which these researches were made. On his return Mr. Huxley contributed two memoirs to the Royal Society, for which the Royal Medal was awarded him. They were printed in the Philosophical Transactions. The publication of the whole of his researches has been strongly recommended by the highest authorities in this branch of science, and will be accomplished by this grant. The work itself will be the property of Her Majesty's Government, and will

be distributed in a manner analogous to that of the Markree and Armagh Star-Catalogues.

9. For the publication of Vol. III. of the Markree Catalogue of Ecliptic Stars	£132	1	7
Vol. III. has been published, and the edition disposed of as already described in the cases of Vols. I. and II.			
10. To Dr. W. B. Carpenter, for completing the illustrations of typical forms of Foraminifera	50	0	0
See note to an appropriation for the same purpose in 1851, No. 6.			
11. To Nevil Maskelyne, Esq., of Oxford, for chemical researches on the solid oils and waxes of the vegetable kingdom..................................	100	0	0
These researches are in progress.			
12. To Dr. Pavy, for continuing experimental researches on the physiology of the blood, of which a part has been recently communicated to the Royal Society .	50	0	0
The researches are in progress.			
13. To Professor William Thomson, for experiments on the thermal effects of electric currents in unequally heated conductors	50	0	0
The experiments are in progress.			
Total sum appropriated in 1854....	£1552	1	7

Total Grants and Appropriations.

GRANTS.	£	s.	d.	APPROPRIATIONS.	£	s.	d.
1850	1000	0	0	1850	1000	0	0
1851	1000	0	0	1851	1000	0	0
1852	1000	0	0	1852	1000	0	0
1853	1000	0	0	1853	620	0	0
1854	1000	0	0	1854	1552	1	7
Replaced by Dr. Stenhouse	100	0	0				
Small balance returned from the Markree and Catton grants	0	10	11				
Totals	£5100	10	11		£5172	1	7

The appropriations exceed the sums placed at the disposal of the Royal Society by £71 10*s.* 8*d.*; which it was the intention of the Council to have provided out of the expected grant of £1000 in 1855.

EDWARD SABINE,
V.P. and Treasurer R.S.

Somerset House, July 20, 1855.

November 15, 1855.

Colonel SABINE, R.A., Treasurer and V.P., in the Chair.

William John Hamilton, Esq., was admitted into the Society.

In accordance with the Statutes, notice was given of the ensuing Anniversary Meeting for the election of Council and Officers.

The Chairman stated that Edward Blackett Beaumont, Esq., who at the late Anniversary Meeting ceased to be a Fellow of the Society in consequence of the non-payment of his subscription, had applied to the Council to be reinstated, alleging that he "had inadvertently omitted to pay his subscription at the proper period." Notice was therefore given that the question of Mr. Beaumont's re-admission would be put to the ballot at the ensuing Meeting of the Society.

A paper was in part read, entitled,—"Experimental Researches in Electricity. Thirtieth Series." By MICHAEL FARADAY, Esq., D.C.L., F.R.S. &c. Received October 24, 1855.

November 22, 1855.

Sir BENJAMIN BRODIE, Bart., V.P., in the Chair.

George Fergusson Wilson, Esq., was admitted into the Society.

In accordance with the notice given at the last Meeting of the Society, the question of Mr. Beaumont's re-admission into the Society was put to the ballot.

The ballot having been taken, Mr. Beaumont was declared to be re-admitted.

In accordance with the Statutes, notice was given of the ensuing Anniversary Meeting; and the following names of noblemen and gentlemen proposed as Officers and Council for the ensuing year were read:—

President.—The Lord Wrottesley, M.A.

Treasurer.—Colonel Edward Sabine, R.A.

Secretaries.— { William Sharpey, M.D. / George Gabriel Stokes, Esq., M.A. }

Foreign Secretary.—Rear-Admiral W. H. Smyth.

Other Members of the Council.—The Duke of Argyll; Neil Arnott, M.D.; Rear-Admiral F. W. Beechey; Sir Benjamin Brodie, Bart.; William Benjamin Carpenter, M.D.; Arthur Cayley, Esq.; Rev. James Challis, M.A.; Charles Darwin, Esq., M.A.; Sir Philip de M. Grey Egerton, Bart.; William Fairbairn, Esq.; John Miers, Esq.; William Allen Miller, M.D.; William Hallows Miller, Esq., M.A.; James Paget, Esq.; John Stenhouse, LL.D.; Rev. Robert Walker.

I. The reading of Dr. FARADAY's paper, "Experimental Researches in Electricity—Thirtieth Series," was resumed and concluded.

The following is an abstract:—

* § 38. *Constancy of differential magnecrystallic force in different media.*—That a magnecrystal formed into a sphere (or some equivalent shape, so that mere length should have no influence) sets with the same force in the magnetic field, whatever the magnetic nature of the medium around it, has been shown generally, and for a few cases, on former occasions. The author was under the necessity of verifying and enlarging the old results; and upon employing the following magnecrystals, namely bismuth, tourmaline, carbonate of

* Series XXIX. is published in the Phil. Trans. for 1852, p. 137.

iron, red ferroprussiate of potassa, and also compressed bismuth, surrounded in succession by the following media,—phosphorus, alcohol, oil, camphine, water, air, and saturated solution of protosulphate of iron, he found the result to be the same as before. The mode of estimating the set was as follows:—The selected crystal being suspended in the magnetic field by a torsion-wire, right-handed force was then slowly applied by the revolutions of the torsion-head above, until the crystal being gradually carried round, attained that position at which any additional torsion-force would cause it to advance suddenly and considerably; this position was called the upsetting point; then left-handed torsion was put on until the like point was attained in the opposite side: the amount of the revolution of the torsion-index from one upsetting point to the other, minus the angle between the upsetting points, was considered as the measure of the set of the crystal under the constant magnetic force employed.

As the setting force of a crystal remained constant for any surrounding medium, it was evidently possible to select a crystal and a medium such, that in one position the crystal would be attracted, and in another, at right angles to the first, be repelled in the same medium. This case was realized with the paramagnetic red ferroprussiate of potassa and a solution of sulphate of iron, and also with the diamagnetic crystal carbonate of lime and diluted alcohol. A crystal was sought for amongst the ferrocarbonates of lime having this relation to the assumed natural zero presented by a vacuum or carbonic acid; but this case was not realized.

§ 39. *Action of heat on magnecrystals.*—When magnecrystals, subjected to the same constant magnetic force, were raised or lowered to different temperatures, it was found that the setting force was affected; and at all temperatures from 0° F. upwards the force diminished as the temperature became higher. Thus the torsion-force of a crystal of bismuth at 92° being 175, was at 279° diminished to 82; that of a tourmaline, by passing from the temperature of 79° to 289°, was so far diminished; that the power at the lower temperature was nearly double that at the higher. A like result occurred with carbonate of iron, and also with compressed bismuth. In all these cases the bodies resumed their first full power on returning to lower temperatures, nor was there any appearance of magnetic charge in any part of the range of observa-

tions. Between 32° and 300° the force of bismuth appeared to alter by regular equal degrees; but with tourmaline and carbonate of iron the change was greatest for an equal number of degrees at the lower temperatures. At a full red heat, however, both tourmaline and calcareous spar retained a portion of their magnecrystallic force or condition, and so did carbonate of iron up to that temperature at which it was decomposed.

It is known that pure calcareous spar points with its optic axis equatorially, but that calcareous spar containing a trace of iron points with its optic axis axially. Calcareous spar retains its magnetic characters at very high temperatures, but carbonate of iron and oxide of iron lose almost the whole of their magnetic force at a dull red heat. It was therefore expected that a ferrocarbonate of lime crystal might become absolutely reversed in condition by change of temperature, and this was found to be the case: at low temperatures the optic axis pointed axially, and at high temperatures equatorially; and that through any number of changes, as the temperature of the crystal was alternately lowered and raised.

§ 40. *Effect of heat upon the absolute magnetic force of bodies.*—Results were sought for, by which the magnetic force of bodies, already examined in the condition of magnecrystals, might be compared with the whole paramagnetic or diamagnetic force of the same bodies taken in the granular or amorphous state; but they were not satisfactory. The carbonate of iron gave the most distinct results; and in its case the change of power by change of temperature was not the same for the two conditions. An examination of the three metals, iron, nickel, and cobalt, at temperatures between 0° and 300° F., gave a very interesting result, which the author is not aware has as yet been noticed. As the temperature rises, the force of the nickel diminishes, the force of the iron remains constant, the force of the cobalt increases; these facts suggest that there is a temperature at which the magnetic force is a maximum, and above or below which it diminishes. The order with the three bodies accords perfectly with that in which they lose the chief amount of their magnetic power, for much loss occurs with nickel at the temperature of boiling oil, with iron at a dull red heat, and with cobalt at a temperature near that of melting copper.

The following communication was also read:—

II. "Discussion of the observed Deviations of the Compass in several Ships, Wood-built and Iron-built; with General Tables for facilitating the examination of Compass-deviations." By G. B. AIRY, Esq., F.R.S., Astronomer Royal. Received September 14, 1855.

[A paper by the author, with the above title, was read on the 21st of June, 1855; it was subsequently withdrawn for the introduction of certain alterations, and, so altered, constitutes the present communication. An abstract is given under the above date, p. 491.]

November 30, 1855.

Anniversary. A Report of this Meeting will appear in a subsequent Number.

December 6, 1855.

Sir BENJAMIN BRODIE, Bart., V.P., in the Chair.

The Chairman announced that the President had appointed the following gentlemen Vice-Presidents:—

Colonel Edward Sabine, R.A.
Rear-Admiral Beechey.
Sir Benjamin Brodie, Bart.
Charles Darwin, Esq.
Sir Philip de M. G. Egerton, Bart.
William Allen Miller, M.D.

The following communications were read :—

I. "On the Determination of the Dew-point by means of the Dry- and Wet-Bulb Thermometers." In a Letter of Lieut. Noble, R.N., of Toronto, to Charles R. Weld, Esq. Communicated by Professor Stokes, Sec. R.S. Received September 24, 1855.

Toronto, September 10th, 1855.

My dear Sir,—The results of the accompanying table for computing the dew-point from readings of the dry- and wet-bulb thermometers, are, as I believe you know, derived from observations taken here during last winter by Mr. Campbell and myself:—

Table *for computing the Dew-point from Readings of the Dry- and Wet-Bulb Thermometers.*

Temperature of air (t).	Factor (f).	Number of observations (m).	Probable error of a single datum (r).	Measure of precision of a single datum (h).	Probable error of the adopted factor $R=\frac{r}{\sqrt{m}}$.	Measure of precision of the adopted factor $H=h\sqrt{m}$.	It is therefore an equal chance that the true factor lies between
48° to 51°	2·31	21	·30	1·590	·07	7·287	2·24 and 2·38
46 ... 47	2·38	13	·26	1·822	·07	6·569	2·31 ... 2·45
42 ... 45	2·53	41	·40	1·189	·06	7·613	2·47 ... 2·59
40 ... 41	2·63	17	·41	1·163	·10	4·796	2·53 ... 2·73
38 ... 39	2·83	25	·48	0·999	·09	4·994	2·74 ... 2·92
34 ... 37	3·02	64	·43	1·114	·05	8·912	2·97 ... 3·07
32 ... 33	3·33	25	·63	·767	·12	3·835	3·21 ... 3·45
30 ... 31	3·81	22	·61	·775	·16	3·633	3·65 ... 3·97
28 ... 29	4·40	27	·66	·723	·13	3 756	4·27 ... 4·53
24 ... 27	5·46	43	·82	·577	·13	3·787	5·33 ... 5·59
22 ... 23	6·06	15	1·20	·397	·31	1·535	5·75 ... 6·37
20 ... 21	6·93	6	1·40	·341	·57	·834	6·36 ... 7·50
18 ... 19	7·13	21	1·44	·331	·31	1·517	6·82 ... 7·44
16 ... 17	7·60	20	1·76	·271	·39	1·209	7·21 ... 7·99
14 ... 15	8·97	17	1·72	·277	·42	1·141	8·55 ... 9·39
12 ... 13	10·30	20	2·53	·188	·56	·842	9·74 ... 10·86
10 ... 11	11·50	11	2·19	·218	·66	·723	10·84 ... 12·16
8 ... 9	13·06	8	4·63	·103	1·64	·292	11·42 ... 14·70
6 ... 7	15·30	7	3·66	·130	1·38	·345	13·92 ... 16·68
0 ... 5	16·23	14	1·87	·255	·50	·955	15·73 ... 16·73
−1 ... −4	19·37	10	4·11	·116	1·30	·367	18·07 ... 20·67
−5 ... −10	21·64	6	4·65	·102	1·90	·251	19·74 ... 23·54
−11 ... −16	37·83	6	10·96	·044	4·48	·107	33·35 ... 42·31

These results will be obvious at a glance; but a few remarks upon the instruments employed, and upon the degree of reliance to be placed upon them, may not be uninteresting.

The dry- and wet-bulb thermometers (for which we were indebted to the kindness of Prof. Cherriman, Director of the Magnetic Observatory, Toronto) were made by Negretti and Zambra, and their index errors were ascertained, above 32° by Mr. Glaisher, and below 32° by ourselves, by comparison with a Kew standard. The divisions upon these thermometers were too small to read 0°·1 with great accuracy; and in discussing our observations at low temperatures, we were in consequence obliged to reject such as would, with an error of 0°·1 in the reading, introduce a considerable error into the factor.

You will observe that the table does not extend below −16°, although we have repeatedly every winter the mercury below −20°, and occasionally below −30°. The only thermometer, however, which we could trust as a wet-bulb in investigations so delicate was not graduated below −16°.

For obtaining the dew-point by direct observation, we used the condensing hygrometer invented by M. Regnault.

We obtained dew with this beautiful instrument at all temperatures (limited only by the graduation of the thermometer −35°), the only requisites when the thermometer is very low being time and pure ether*. I can testify from experience that this hygrometer obviates all the disadvantages of Daniell's, which M. Regnault enumerates in his hygrometrical researches.

In order to show the reliance that may be placed upon our results, we have put opposite each factor in the table the probable error and measure of precision of the single data (from which the factor (f) was derived), and also the probable error, measure of precision, and limits of certainty of the adopted factor. The nomenclature and notation are thus employed by Encke in his Memoir on the Method of Least Squares.

The measure of precision (h), as was indeed to have been expected, decreases with the temperature. This fact is not however of so much importance as might at first appear.

* The ether we employed below −20° was the first that passed over, resulting from the distillation of washed ether with quicklime.

For the dew-point is given by the equation,—

$$T=t-f(t-t');$$

where (T) is the temperature of the dew-point, (t) that of the air, ($t-t'$) the difference between the dry- and wet-bulb thermometers, and (f) the factor whose value is given in the table.

Now taking the temperatures 42° and 22°, it appears from the table that the probable error of (f) for a single observation is at the latter temperature three times greater than at the former. But ($t-t'$) is on an average about three times as great at 42° as at 22°. Hence the probable error of the dew-point at both temperatures is very nearly the same.

We have extended our table to 51° for the purpose of comparison with the "Greenwich factors." I must however remark, that it is probable that the factors, which we have given above 40°, are rather greater than they would have been had the observations discussed extended through a longer space of time, the majority at these temperatures having been taken last spring, when the air was very remarkably dry; and experience shows that when ($t-t'$) is unusually great, the deduced factor, instead of being more accurate, is generally much too large.

As an instance, I may cite an observation taken on April 29th, when the temperature of the air was 43°·6, that of evaporation was 31°·6, and that of the dew-point 3°·2. The fraction of saturation on this occasion was $\frac{19}{100}$, and the factor derived from this observation was 3·36; this being much the largest deviation from the adopted mean 2·53.

The cause of this discrepancy is doubtless owing to the heat that the wet-bulb thermometer derives from the radiation of surrounding objects; and were observations sufficiently numerous, it might conduce to accuracy were the factors calculated for every degree of difference in the value of ($t-t'$).

We purpose instituting a comparison between two wet-bulb thermometers placed in similar boxes, the one box coated with lampblack, the other with a polished metallic surface.

Below 32° our results do not appear to coincide with the factors deduced from the Greenwich observations; and the causes of these discrepancies I must leave to time.

As, however, we have had considerable experience at these temperatures, I may perhaps be doing service to observers in bringing before their notice two causes of error, to which we have found ourselves particularly liable when the thermometer is near 32°.

1st. If the air is a little above, and has been below 32°, there will frequently be a small button of ice at the foot of the wet-bulb thermometer, which is not easily perceived, and which will keep it at 32° when the temperature of evaporation is really above that point.

2ndly. It is well known that under certain circumstances water may be cooled below 32° without freezing; and an example will perhaps best show the error which this fact may occasion.

Let us suppose that the temperature of the air is 27°, and that when the thermometer is wetted it sinks to 26°, and then rises. Should it rise very slowly, or not at all, the probability is that 26° is the true temperature of evaporation, but if rapidly, the rise may be due to the conversion of the water into ice; and it will be prudent to observe whether or not the thermometer again commences to sink.

We have frequently observed this phenomenon, and I am quite at a loss to what to ascribe its uncertainty.

It has occurred both in a high wind and a calm (the thermometers are protected from the full force of the wind), and it also appeared to be quite uncertain at what temperature the water might freeze.

I am obliged to admit that the limits of certainty of the factors below zero are not quite so close as could be desired. This is partly attributable to our being obliged to reject many observations made with a thermometer which was broken before its index-errors were fully ascertained; but Mr. Campbell and I must claim the indulgence of those who know the difficulty of taking observations requiring so much time and accuracy at such temperatures, and frequently at six o'clock in the morning.

Believe me, &c.,

W. Noble,

Lt. R.N.

C. R. Weld, Esq., Assist. Sec. Royal Soc.

II. "On Chemical Affinity, and the Solubility of the Sulphate of Baryta in Acid Liquors." By F. Crace Calvert, Esq. Communicated by William Allen Miller, M.D., F.R.S. Received October 27, 1855.

Solubility of the Sulphate of Baryta.

The author observes that sulphate of baryta is not an insoluble salt, as is generally admitted, for he has found that 1000 grs. of nitric acid, of spec. grav. 1·167, are capable of dissolving 2 grs. of sulphate of baryta; and what renders the knowledge of this fact still more useful in analytical chemistry is, that the insolubility of this salt is affected even by the weakest nitric or hydrochloric acids; for whilst 0·062 gr. of sulphate of baryta only requires 1000 grs. of nitric acid, of spec. grav. 1·032, to hold it in solution, the same quantity of salt requires 50·000 grs. of pure distilled water to dissolve it.

What is not less useful to know is, that the solubility of sulphate of baryta is affected in a higher degree by the bulk of the acid than by its strength. The two following tables, taken from amongst many others contained in the paper, will not only illustrate this fact, but will also give an insight into the way in which the experiments were conducted. The first table illustrates the influence which increasing bulks of the same nitric acid exert on the formation of sulphate of baryta, and the second table the action which increasing strengths of acid have:—

Table XVI.

Order of jars.	Number of divisions of the alkalimeter of nitric acid, spec. grav. 1·167.	Number of divisions of the alkalimeter of water added.	Spec. grav. of the bulk of acid.	Sulphate of potash dissolved in the acid.	Nitrate of baryta poured in, previously dissolved in 20 grs. of water.	Time required for a precipitate to appear.	Quantity of sulphate of baryta precipitated.	Quantity of sulphate of baryta dissolved.
1	20	20	1·167	3·34	5·00	3 min.	4·28	Average
2	20	40	1·120	...	...	...	4·34	quantity
3	20	60	1·085	...	...	...		dissolved
4	20	80	1·067	...	...	...		equal
5	20	100	1·057	...	...	...	4·35	0·10 gr.
6	20	120	1·050	...	...	...	4·35	
7	20	140	1·044	...	...	...	4·36	
8	20	160	1·039	...	...	...		
9	20	180	1·035	...	...	...		
10	20	200	1·032	...	...	...	4·38	

TABLE II.

Order of jars.	Number of divisions of the alkalimeter.	Corresponding weight of nitric acid, sp. gr. 1·167.	Quantity of sulphate of potash.	Quantity of nitrate of baryta.	Weight of sulphate of baryta.	Time required for a precipitate to appear.	Quantity of sulphate of baryta dissolved.
1	40	466·8	3·34	5·00	4·46	Instantly	0·02
2	80	933·6	...	...	...	20 minutes	1·29
3	120	1400·4	...	...	...	2 hours	2·34
4	160	1867·2	...	...	...	$8\frac{1}{2}$ hours	3·66
5	200	2334·0	...	...	...	24 hours	
6	240	2800·8	...	...	...	No precip.	
7	280	3267·6	...	...	...		
8	320	3734·4	...	...	...		
9	360	4201·2	...	...	...		
10	400	4668·0	...	...	...		

These tables clearly show the influence which a given strength of nitric acid has on the solubility of the sulphate of baryta; for there is a precipitate in three minutes in all the jars of the first table, whilst we have a precipitate only in the first four jars of Table II.

Another fact which is observed in these tables is, that whilst 240 divisions of the alkalimeter of nitric acid, spec. grav. 1·167, are capable of dissolving, or preventing the formation of, 4·46 grs. of sulphate of baryta, 240 divisions of an acid, of spec. grav. 1·032, only retained in solution 0·086 gr. It follows from these facts, that in future the practice of rendering liquors acid with nitric or hydrochloric acids, must be discontinued when sulphates are to be determined, or separated from chromates, phosphates, &c.

Influence of Mass on Chemical Affinity.

The researches of the author, to illustrate the influence which mass exerts on chemical affinity, are extensive; a few of the results arrived at are here given.

The following table will clearly show the marked influence which increasing volumes of nitric acid have in preventing the formation of sulphate of baryta :—

TABLE IV.

Number of jars.	Number of divisions of the alkali-meter.	Corresponding weight of acid, sp. gr. 1·167.	Quantity of sul-phate of potash.	Quantity of nitrate of baryta.	Weight of sulphate of baryta.	Time when precipitate appeared.
1	40	466·8	5·12 grs.	8·00	7·13	Instantly
2	80	933·6	...	...	...	2 minutes
3	120	1400·4	...	...	...	14 minutes
4	160	1867·2	...	...	...	1 hour
5	200	2334·0	...	...	...	1 hour 15 minutes
6	240	2800·8	...	...	...	4 hours
7	280	3267·6	...	...	...	8 hours [cloud)
8	320	3734·4	...	...	...	21 hours (only a
9	360	4201·2	...	...	...	None
10	400	4668·0	...	...	...	None

Thus in this table we perceive, that as the bulk of acid increases, more time is required for a precipitate to appear, although there is a large excess of substance employed on the quantity necessary to give an instantaneous precipitate ; and it is curious to observe how wide is the space of time in each successive jar for a precipitate to appear, and in jars numbers 9 and 10 no deposits were formed after twenty-four hours. As the quantity of precipitate decreased rapidly in each successive jar, they were gathered, and their amount determined with due care ; and these are the facts observed :—

TABLE V.

Number of jars.	Number of divisions of the alkali-meter.	Correspond-ing weight of nitric acid, sp. gr. 1·167.	Quantity of sulphate of baryta preci-pitated.	Quantity of sulphate of baryta dis-solved.	Quantity of sul-phate of baryta dissolved per 1000 grs.
1	40	466·8	6·86	0·27	0·591
2	80	933·6	5·63	1·50	1·615
3	120	1400·4	4·66	2·47	1·767
4	160	1867·2	3·22	3·91	2·099
5	200	2334·0	2·33	4·80	2·059
6	240	2800·8	1·10	6·03	2·155
7	280	3267·6	0·14	6·99	2·141

The results contained in this table, especially those in the last column, clearly show the influence of mass on chemical affinity, for there is no difference in any of the jars excepting the increasing bulk of the acid ; and still we have in jar No. 1 only 0·591 of sulphate of baryta dissolved per 1000 grs. of acid, whilst we have 2·099 in No. 4 jar.

But the relative bulk of acid is not the only influence which affects the affinity of sulphuric acid for baryta, as the relative quantities of nitrate of baryta and sulphate of potash put in presence also exert an action. This fact is illustrated by the following results, taken from three different tables, in which the same quantities of acid were used, but different proportions of salts :—

TABLE IV. E.

Number of table.	Quantities of acid, sp. gr. 1·167.	Sulphate of potash.	Nitrate of baryta.	Sulphate of baryta.	Time before a precipitate appeared.
No. 1	466·8	0·753	1·121	1·00	12 hours
	933·6	0·753	1·121	1·00	None
No. 2	466·8	3·34	5·00	4·46	Instantly
	933·6	3·34	5·00	4·46	2 hours
No. 4	466·8	5·12	8·00	7·13	Instantly
	933·6	5·12	8·00	7·13	2 minutes

This point is again brought out in the following table, which is also taken from several series of experiments :—

TABLE IV. A.

Number of table.	Order of jar.	Quantities of fluid.	Total quantities of sulphate of baryta susceptible of being produced.	Quantity of acid represented.	Quantities of sulphate of baryta dissolved in 1000 grs.	Increased ratio of solubility.
No. 1	2	933·6	1·00	1000	1·071	0·
No. 2	6	2800·8	4·46	...	1·593	0·522
No. 4	9	4201·2	7·13	...	1·912	0·841

These facts, and others described in the paper, demonstrate that the solubility of the sulphate of baryta, or its non-formation, is not only influenced by the respective bulks of an acid of spec. grav. 1·167, and the respective quality of salts employed, but that the relative quantity of matter put in presence has a decided influence on chemical affinity ; and these observations not only corroborate perfectly the results obtained by Mr. Bunsen on the influence of volumes on the combination of gases, and the observations which show a like influence on the carbonates, but also are, I believe, the first instance which has been noticed of irregularity of solubility of a substance in increased multiple bulks of a liquid.

III. "Results of the Examination of certain Vegetable Products from India."—Part I. By John Stenhouse, LL.D., F.R.S. Received November 14, 1855.

(Abstract.)

Through the kindness of my esteemed friend Dr. Royle, I have been permitted to select such vegetable products from the extensive collection at the India House as seemed most likely to repay the trouble of investigation. My attention, during the last twelve months, has been chiefly directed to three of these vegetable substances; and the results of their examination I now take the liberty of submitting to the Royal Society, to be followed by those of the others as they may be completed.

Datisca cannabina.

The first of these substances which I examined consisted of a quantity of the roots of the *Datisca cannabina*, from Lahore, where this plant is employed to dye silk of a fast yellow colour. The roots, which had been cut into pieces about 6 or 8 inches long, were from a half to three-quarters of an inch in thickness. They had a deep yellow colour. A decoction of the leaves of the *Datisca cannabina* was examined by Braconnot in 1816, who discovered in it a crystallizable principle, to which he gave the name of *datiscine*. Braconnot, of course, did not subject this substance to analysis, but he described its appearance and properties in an exceedingly accurate manner*. The observations of Braconnot had fallen into such entire oblivion, however, that for many years past, we find in most of the larger systems of chemistry the term datiscine used as synonymous with inuline. Thus in Brande's 'Chemistry,' vol. ii. p. 1168, we find it stated that a variety of names had been given to inuline, such as "dahline, datiscine," &c.

The bruised roots were extracted in a Mohr's apparatus by long-continued digestion with wood-spirit. The liquor obtained, which had a dark brown colour, was concentrated by distilling off a portion of the wood-spirit. The brown syrupy liquid remaining in the retort, on being poured into open vessels and standing for some

* Annales de Chimie et de Physique, 1816, iii. 277.

time, deposited a resinous matter containing merely traces of a crystalline substance. When this syrupy liquid, however, was treated with about half its bulk of hot water, the greater portion of the brown resin was rapidly deposited, and the mother-liquor having been poured off and left to spontaneous evaporation, deposited a considerable quantity of an imperfectly crystallizable substance resembling grape-sugar. These crystals are datiscine containing a considerable amount of resinous matter. The datiscine, however, is rendered perfectly pure by treatment with a solution of gelatine, to remove any trace of tannic acid, and repeated crystallizations out of weak spirits of wine.

Properties of Datiscine.—Datiscine, when pure, is perfectly colourless. It is very soluble in alcohol, even in the cold, boiling alcohol dissolving any amount of it. By slow spontaneous evaporation, its alcoholic solutions yield small silky needles arranged in groups. Cold water does not dissolve much of it, but it is tolerably soluble in boiling water, the hot solutions on cooling depositing it in shining scales.

Datiscine is not very soluble in ether; but an ethereal solution, when evaporated, yielded larger crystals than were obtained by any other method. On adding water to an alcoholic solution of datiscine, no precipitate is immediately obtained, unless the solution is greatly concentrated; but on standing, very pure, pale yellow-coloured crystals of datiscine separate.

When datiscine is heated to about 180° C., it melts, and if the heat be increased, it burns, evolving an odour of caramel, and leaves a voluminous charcoal. If datiscine be heated in a close vessel while a stream of dry air is slowly passed over it, a small quantity of a crystalline substance sublimes. Datiscine and its solutions have a very bitter taste; and though it does not produce any change on test-paper, I think there is reason to regard it as a feebly acid body.

It dissolves in solutions of the fixed alkalies and ammonia, also in lime- and baryta-water. The addition of an acid to these solutions causes the precipitation of the datiscine.

The aqueous solution of datiscine is precipitated by neutral and basic acetates of lead, and chloride of tin. These precipitates have a bright yellow colour. Salts of copper produce greenish, and those of peroxide of iron brownish-green precipitates. In con-

sequence of the lead salts forming such gelatinous precipitates, they could not be employed for determining the equivalent of datiscine.

Action of dilute Sulphuric Acid on Datiscine.—When an aqueous solution of datiscine is boiled for a few minutes with very dilute sulphuric acid, it deposits a crystalline substance. On examining the solution filtered from the crystals, very distinct evidences of the presence of sugar were obtained. These experiments show therefore that datiscine, like salicine and similar bodies, belongs to the class of glucosides, and is a copulated compound of sugar and another substance which I shall call "datiscetine."

Datiscetine.—Datiscetine in its general appearance and properties closely resembles datiscine, but on a closer examination these two substances are found to differ essentially both in composition and properties. Datiscetine when pure assumes the form of fine needles which are nearly colourless. It is easily soluble in alcohol, a hot alcoholic solution, on cooling, depositing the greater portion in crystalline groups. It is almost insoluble in water, consequently datiscetine is precipitated from its alcoholic solutions by the addition of water. It dissolves in ether in almost any quantity, and is deposited on the evaporation of that liquid in needles. These properties of datiscetine enable us to obtain it in a tolerably pure state, even when very impure datiscine is employed in its preparation.

Properties of Datiscetine.—Datiscetine has no taste. When heated it melts like datiscine, but the heat required is much higher than for that body. It recrystallizes on cooling. By operating very cautiously, a portion of the datiscetine may be sublimed. The sublimate, however, appears to be altered datiscetine. Datiscetine when burned does not emit the odour of caramel. Datiscetine, like datiscine, dissolves in alkaline solutions, and is reprecipitated by the addition of an acid. An alcoholic solution of acetate of lead added to an alcoholic solution of datiscetine produces a deep yellow precipitate, which can be easily washed both by alcohol and water. This precipitate therefore was subjected to analysis, and from the results obtained, the formula $C_{30} H_8 O_{10} + 2PbO$ was calculated, which agrees with the formula ($C_{30} H_{10} O_{12}$) derived from the analysis of datiscetine.

Analysis of Datiscine.—It is difficult to calculate a formula for

datiscine, the numbers of which shall agree with those found by analysis. When, however, the decomposition of datiscine into datiscetine and sugar is taken into consideration, it seems probable that the formula for datiscine is

$$\begin{array}{ccccc} \text{Datiscetine} & + & \text{sugar} & = & \text{datiscine} \\ C_{30}H_{10}O_{12} & + & C_{12}H_{12}O_{12} & = & C_{42}H_{22}O_{24}. \end{array}$$

If the formula $C_{42}H_{22}O_{24}$ be correct, the decomposition of datiscine by dilute sulphuric acid would be analogous to that of salicine when treated in the same way.

Dilute hydrochloric acid, like dilute sulphuric acid, decomposes datiscine, converting it into datiscetine and sugar. On boiling an aqueous solution of pure datiscine for some hours, traces of sugar could be detected, thus showing that a small portion of the datiscine had been decomposed.

It has been already shown that datiscine dissolves in cold solutions of potash without decomposition. When boiled, however, with a strong solution of potash for some time, decomposition takes place, and the precipitate, thrown down by the addition of an acid, has all the properties of datiscetine. In this respect, therefore, datiscine agrees with tannin and similar glucosides, which yield the same products when acted upon by acids and alkalies. Yeast and emulsine appeared to exert no action on solutions of datiscine.

Action of Nitric Acid on Datiscine and Datiscetine.—Cold nitric acid of the ordinary strength acts violently upon datiscetine, brown vapours are disengaged, and a resinous substance is produced, which is ultimately dissolved, forming a dark red liquid, which, when evaporated, yields crystals of nitropicric acid.

Datiscine treated in the same way yields nitropicric and oxalic acids.

When datiscine is boiled with dilute nitric acid it dissolves, and the solution obtained, when cooled, deposits pale yellow crystals, which agree in every way with the properties ascribed to nitrosalicylic acid.

On allowing datiscine to stand in contact with dilute nitric acid in the cold it gradually dissolves, the solution, when left to evaporate *in vacuo*, depositing a mixture of oxalic and nitropicric acids.

Action of Potash on Datiscine and Datiscetine.—It was stated in a previous part of this paper that datiscine and datiscetine dissolve in cold solutions of the alkalies without decomposition, and that datiscine, when boiled with potash, is decomposed with the formation of datiscetine. It only remained, therefore, to try the action of fused hydrate of potash. Datiscetine, when added in small successive portions to fused hydrate of potash, assumed a deep orange colour, and then dissolved with the evolution of hydrogen gas. When the disengagement of hydrogen had ceased, the mass was dissolved in water and supersaturated with hydrochloric acid. A partly resinous substance separated, which, by sublimation, yielded perfectly colourless, long crystals closely resembling benzoic acid. Their solution in water on the addition of perchloride of iron gave that deep violet tint which disappears on the addition of hydrochloric acid, and is so characteristic of salicylic acid.

Action of Chromic Acid on Datiscetine.—On distilling datiscetine with bichromate of potash and sulphuric acid a liquid came over, containing no oily drops, but having the smell of salicylous acid, and which, when tested with a persalt of iron, formed a purple-coloured solution characteristic of that acid.

It follows therefore, I think, from the experiments already detailed, that datiscine, like salicine, phloridzine, &c., is a glucoside, and that it approaches nearer to salicine than any other glucoside, with the exception of populine, yet known.

I will conclude this account of datiscine by proposing the following practical application. As is well known, the colouring matter of madder, when boiled with dilute sulphuric acid, is changed into sugar and garancine, a new dye-stuff, which, for many purposes, is found superior to that originally present in the madder. Within the last twelve months Mr. Lieshing, by treating the colouring matters in weld and quercitron bark with dilute sulphuric acid, has resolved them into new colouring matters, which are but slightly soluble in water, and are found nearly three times more powerful as dye-stuffs than the original colouring matters from which they had been produced.

As datiscine, when boiled with dilute sulphuric acid, undergoes a perfectly similar transformation, being resolved into sugar and datiscetine, which has a much higher colouring power than the

datiscine which has produced it, I have not the least doubt that silk dyers, who may hereafter employ solutions of *datisca cannabina,* will find it highly advantageous to convert their datiscine into datiscetine by boiling it with dilute sulphuric acid.

Oil of the Ptychotis Ajowan.

The *Ptychotis Ajowan* is an umbelliferous plant well known in India for its aromatic and carminative properties. When its seeds are distilled with water they readily yield between five and six per cent. of an essential oil resembling in smell that of oil of thyme. When this oil is left in shallow vessels to spontaneous evaporation at low temperatures, it deposits a large quantity of beautiful crystals, which are identical with the stearopten brought from India by the late Dr. Stocks, and described by me in a short notice in the December Number of the 'Pharmaceutical Journal' for 1854.

The crude oil was rectified, and the portion which came over between 160° C. and 164° C. was collected separately and carefully rectified over sodium. Its boiling-point was found to be 172° C., and its composition isomeric with oil of turpentine, namely

$$C_{10}\,H_8 \text{ or } C_{20}\,H_{16}.$$

The stearopten obtained from the less volatile portion of the oil was purified, and formed large flat rhombohedral crystals, which have been carefully measured by Professor Miller of Cambridge. When subjected to analysis it gave the formula

$$C_{20}\,H_{14}\,O_2 \text{ or } C_{20}\,H_{13}\,O_1\,HO.$$

In the notice of the stearopten of this oil which appeared in the 'Pharmaceutical Journal,' from the examination of the small quantity then at my disposal, a different formula was deduced, which I now withdraw, and substitute the preceding formula, $C_{20}\,H_{14}\,O_2$, in its stead.

When the crystalline stearopten is digested for eight or nine days with the strongest nitric acid, it is gradually converted into a crystallizable acid, apparently containing no nitrogen. This acid is but slightly soluble even in boiling water, but is deposited in needles on the cooling of the solution. It is very soluble in alcohol and ether, from which it is deposited in crystals. Both its silver and its baryta salts are crystallizable, and appear very stable. It

seems to me to be a new acid, and this I hope soon to be able to ascertain.

When the stearopten is gently warmed with oil of vitriol it dissolves, and, on cooling, solidifies into a crystalline mass. This new compound, which is a copulated acid, dissolves readily in hot water, and, on cooling, is deposited in large scaly crystals, of a mother-of-pearl lustre. It forms both a crystallizable lead and baryta salt.

When the stearopten is distilled with a mixture of peroxide of manganese and sulphuric acid, it yields a substance exceedingly analogous in almost every respect to the thymöil obtained by Lallemande by subjecting the stearopten of oil of thyme to similar treatment. As the details of Lallemande's experiments have not yet been published, it would be premature to pronounce with absolute confidence on the identity of the stearoptens from the *Ptychotis* and oil of thyme; but if not identical, as I rather apprehend they are, they are certainly extremely similar bodies*.

Gum of the Gardenia lucida, *Roxb.* (*the Decamalee Gum of Scinde*).

The specimen of this gum on which I operated was evidently very old. It formed a hard, dry mass, of a dark brown colour, with numerous patches of a greenish-yellow matter disseminated through it. It had but a faint odour, unless freshly fractured or gently heated, when it smelt like the urine of the cat.

A comparatively recent specimen of this gum, which I saw in the hands of the late Dr. Stocks, had merely the consistence of candied honey, and an exceedingly offensive odour. Dr. Stocks informed me that the fresh gum was employed as a dressing for wounds, as it kept off the flies. The resin was digested in strong spirit of

* Since this paper was communicated to the Royal Society, a notice of the Ptychotis oil, by Dr. Haines, of the Bombay College, was read before the last meeting of the Chemical Society. Dr. Haines has generally arrived at similar conclusions to my own. He regards, however, the carbohydrogen portion of the oil not as isomeric with oil of turpentine, but as $C_{20} H_{14}$.

His formula for the stearopten is the same as that given in this paper; and he regards it as identical with Lallemande's thymole.

Dr. Haines, however, appears not to have observed the crystalline acid produced by the action of nitric acid on the stearopten.

wine, till a saturated solution was obtained. This, on cooling, immediately deposited some yellow amorphous flocks. These were separated by filtration, and the clear liquid slowly evaporated *in vacuo*. On standing a few days, it deposited a quantity of golden yellow slender crystals, about half an inch in length. The crystals had considerable lustre, and were very brittle. To this crystalline substance I purpose giving the provisional name of Gardenine. Gardenine is nearly insoluble both in cold and hot water. It dissolves pretty readily in alcohol, but much less readily in ether; ether yielding bright yellow solutions, out of which it crystallizes on cooling. Alkalies, such as ammonia, do not appear to increase its solubility. It is more soluble in hot hydrochloric and sulphuric acids than in water, and is precipitated, apparently unchanged, on the addition of water. Its alcoholic solutions give no precipitate with ammonio-nitrate of silver, or with basic acetate of lead. When gardenine is digested with concentrated nitric acid, it is rapidly decomposed; nitropicric acid, but apparently no oxalic acid, being produced.

Unfortunately, from the very small quantity of resin at my disposal, I was unable to prepare a sufficient amount of the gardenine either to subject it to analysis or to examine it more particularly. Dr. Royle has, however, commissioned a large quantity of the resin from India, which I trust will ere long enable me to complete its examination. Gardenine appears to belong to the tolerably numerous class of indifferent crystallizable resins, of which it is certainly one of the most beautiful.

IV. "On the Representation of Polyhedra." By the Rev. Thomas P. Kirkman, A.M. Communicated by Arthur Cayley, Esq., F.R.S. Received August 6, 1855.

This paper constituted an addition to the paper by the same author read June 21, 1855.

The author observes that to every p-acral q-edron corresponds a p-edral q-acron, the summits and faces of either having the same order and rank as to the number of edges with the faces and summits of the

other. When $p=q$, the corresponding pair will sometimes be identical figures, as to the number, rank, and arrangement of faces and summits; and at other times, as is always the case if p be not equal to q, the two figures will differ. When they differ they may be called a *sympolar* pair, both being *heteropolar*; when they form one and the same figure it may be styled an *autopolar* polyhedron. An elegant way of representing a sympolar pair is deduced from the two following theorems:—

A. The q summits of a q-acron are the angles of a closed polygon of q sides, all edges of the q-acron.

B. A closed polygon of p sides can be traced on the p faces of every p-edron, having a side in every face, and passing through no summit.

December 13, 1855.

Col. SABINE, R.A., Treas. and V.P., in the Chair.

It was announced that Mrs. Young, widow of Thomas Young, M.D., For. Sec. R.S., had presented to the Society a volume of MS. letters addressed to her husband by MM. Arago, Fresnel, Poisson, La Place, A. Humboldt, Berzelius, Biot, Bessel, and Dr. Wollaston.

The following communications were read:—

I. "On the Action of Sulphuric Acid on the Nitriles and on the Amides." By G. B. Buckton and A. W. Hofmann, Ph.D., F.R.S. Communicated by Dr. Hofmann. Received December 1, 1855.

Although the identity of the nitriles with the hydrocyanic ethers has been established by the experiments of M. Dumas, who obtained them by the action of anhydrous phosphoric acid upon certain ammoniacal salts, chemists have hitherto in vain sought for a method by which a passage might be effected from the nitriles to the general alcohol derivatives.

Our attention has been directed likewise to the same subject, but

our exertions, like those of our predecessors, have not yet been crowned with success. The researches on which we have been engaged, have, however, furnished some results which appear to us worthy of the interest of chemists, the reactions being at the same time remarkable for their neatness and susceptibility of general application.

Acetonitrile may be considered as the type of its class, both from the importance of the group to which it belongs, and the facility of its preparation. It is, in fact, the nitrile with which we have been specially engaged. This body when mixed with its own volume of fuming sulphuric acid, evolves a considerable amount of heat, accompanied by a very energetic reaction.

If care be taken to add the acid in small quantities, and to cool the mixture after each addition, scarcely any change of colour is to be observed. By the addition of water, and subsequent saturation with carbonate of barium, a crystalline salt is produced in considerable quantity, which possesses all the characters and composition of the sulphacetate of barium,

$$C_4\,(H_2\,Ba_2)\,O_4\,2\,SO_3,$$

discovered some time ago by M. Melsens in the mutual reaction of anhydrous sulphuric acid upon glacial acetic acid.

If, on the contrary, the mixture of acetonitrile and fuming sulphuric acid be made rapidly, and the liquid be rather strongly heated, an abundant evolution of carbonic acid indicates a more profound reaction. The residuary mass, which is tough and of a resinous consistency, when treated with water, and boiled with excess of carbonate of barium, yields a magnificent crystallization of a salt represented by the formula

$$C_2\,(H_2\,Ba_2)\,4\,SO_3 + 4aq.$$

It is a substance of remarkable stability. It does not lose weight at a temperature of 100° C., but four equivalents of water of crystallization are disengaged at 170° C. It is not further affected by a temperature of 220° C. A strong heat resolves it into water, sulphide of barium, sulphurous acid, free sulphur, and carbonic oxide. It may be boiled for hours with fuming nitric acid without the slightest decomposition.

We have also made analyses of the ammonia and silver salts. The former crystallizes with great facility in colourless oblique

prisms, which may be readily obtained upwards of an inch in length. They are anhydrous, and perfectly stable at a temperature of 190° C.

The composition of this salt is represented by the formula

$$C_2\,[H_2\,(NH_4)_2]\;4\,S\,O_3.$$

The silver salt is obtained by digesting oxide of silver with an aqueous solution of the new acid. It forms large crystals, which are easily soluble in water, but insoluble in alcohol or ether. They contain

$$C_2\,(H_2\,Ag_2)\;4\,SO_3.$$

The acid may be obtained by decomposing either the lead or silver salt by hydrosulphuric acid, or perhaps more conveniently, by carefully precipitating a solution of the barium salt with sulphuric acid. It is an exceedingly soluble and deliquescent substance, crystallizing in long needles. It has a sharp acid taste, with somewhat of the flavour of tartaric acid.

For want of a more convenient name, we propose for this acid the appellation of Methylo-tetra-sulphuric acid. Without wishing to decide at present upon its constitution, its composition allows us to consider it as formed by the association of marsh gas with four equivalents of anhydrous sulphuric acid.

In the reaction of sulphuric acid upon acetonitrile, two distinct phases may be traced. In the first, the nascent acetic acid simply combines with two equivalents of sulphuric acid; in the second phase, the acetic molecule undergoes a more thorough transformation; faithful to its tradition it splits into carbonic acid and marsh gas, which remains combined with four equivalents of sulphuric acid. The new substance also may be regarded as sulphacetic acid, which, losing carbonic acid, has assimilated an equal number of equivalents of sulphuric acid.

The two reactions may be represented by the following equations:—

$$\underbrace{C_4\,H_3\,N}_{\text{Acetonitrile.}} + 2\,H\,O + 3\,H\,SO_4 = \underbrace{C_4\,H_4\,O_4\;2\,SO_3}_{\text{Sulphacetic acid.}} + N\,H_4\,SO_4.$$

$$\underbrace{C_4\,H_3\,N}_{\text{Acetonitrile.}} + 5\,H\,SO_4 = \underbrace{C_2\,H_4\;4\,SO_3}_{\substack{\text{Methylo-tetra-}\\\text{sulphuric acid.}}} + N\,H_4\,SO_4 + 2\,CO_2.$$

The action, then, of bases and of acids upon acetic acid presents a remarkable analogy.

The nature of the change which the acetic molecule suffers, is in fact identical. Under the influence of both agents, it splits into marsh gas and carbonic acid, but in the former case it is the carbonic acid which is fixed, whilst in the latter it is the marsh gas that remains in combination.

The production of methylo-tetra-sulphuric acid calls to mind the interesting substance, sulphate of carbyl, discovered by M. Magnus, by combining olefiant gas with the vapour of anhydrous sulphuric acid. Our new acid is however easily distinguishable from this body, as well by its different composition as by its extreme stability; sulphate of carbyl, as is well known, being decomposed by water into isethionic acid.

As acetamide differs from acetonitrile only in containing two additional equivalents of water, it undergoes with Nordhausen sulphuric acid a strictly analogous transformation.

From the comparative facility of its preparation it offers peculiar advantages for procuring the methylo-tetra-sulphates. The only difference to be noted is, that in this case the ammonia salt is generally eliminated instead of the free acid.

M. Melsens, in his researches upon the sulphacetates, appears in some sort to have anticipated the existence of the methylo-tetra-sulphates. He remarks that he once found in the mother liquor obtained from the preparation of sulphacetate of silver, a crystalline salt, the composition of which he represents by the formula

$$C_2\,H_2\,Ag_2\,S_4\,O_{12}.$$

It is evident that these crystals contain the same elements as methylo-tetra-sulphate of silver, but M. Melsens does not appear to have investigated the subject further than by showing the existence of this silver salt.

A detailed description of the methylo-tetra-sulphates, and the study of the corresponding bodies of other series, will be on our part the subject of a special memoir.

II. "On the Structure and Development of the Cysticercus cellulosæ, as found in the Pig." By GEORGE RAINEY, Esq., Lecturer, and Demonstrator of Practical and Microscopical Anatomy at St. Thomas's Hospital. Communicated by ROBERT D. THOMSON, M.D., F.R.S. Received June 27, 1855.

(Abstract.)

The Cysticercus cellulosæ, in its mature state, consists of two parts: one a small oval cyst, composed of a very thin membrane, rendered uneven on its external surface by minute rounded projections, and containing in its interior, granular matter, particles of oil, and a colourless fluid. This may be called its ventral portion. The other is folded inwards, occupying the centre of the cyst just described, but by pressure it may be made to protrude. This part is sometimes called the neck. Its length varies very much in different cysticerci, depending upon their age. It is hollow, having strong membranous parietes, wrinkled transversely, and composed both of circular and longitudinal fibres. The cavity has no visible communication with that of the ventral portion. It contains a multitude of small oval laminated calcareous bodies, which, when acted upon by acids, effervesce briskly, and become partially dissolved, leaving only a small residue of animal matter. When the neck is protruded, the extremity farthest from the cyst is seen to present an enlargement, sometimes called the head, on the free surface of which there is a quadrangular area, occupied by four circular disks and a ring of hooklets. Each angle contains a disk, and the hooklets are placed in a circle around the centre of this space. The suctorial disks are traversed each by a passage taking rather a spiral course, and terminating in the cavity of the neck. The membrane composing a disk presents two orders of fibres, circular and radiating. The hooklets are generally twenty-six in number, thirteen long and as many short, arranged alternately a long and a short one. Each consists of a curved portion like a bird's claw, and a straight portion or handle; and at the junction of these two parts there are tubercles, two in the short hooklets, and only one in the long ones. The hooklets are crossed by two zones of circular fibres. They are also connected by

radiating fibres, which occupy the spaces between each adjacent pair, like the interosseous muscles situated between the metacarpal bones and phalanges. The hooklets are disposed like radii, with their points turned outwards and the extremities of their handles inwards, which, not meeting, circumscribe a circular space whose centre corresponds to that of the quadrangular area before mentioned. At this part there is no perforation answering to an oral orifice, but here the membrane is simply depressed so as to present a conical hollow. By pressure upon the neck, this membrane can be made to protrude in the form of a tongue-like process, to which the handles of all the hooklets are connected, so that when this part in the living animal is made to move, the handles of the hooklets will be drawn in with it, and their points carried from the entozoon, and thus made to penetrate the part to which it attaches itself. These entozoa are chiefly found in the cellular intervals between the muscular fibres, contained in an adventitious cyst formed by the condensation of the surrounding tissues. No more than one entozoon is ever met with in one cyst.

Development of the Cysticercus cellulosæ.

The earliest appearance of the incipient stage of the cysticercus cellulosæ is a fusiform collection of small cells and molecules in the substance of a primary muscular fasciculus, or immediately beneath its sarcolemma. These cells, in this condition of the entozoon, have only an imperfect or partial covering; however, they soon become completely enclosed in a well-defined membrane which is at first homogeneous, but which afterwards sends out short, slender, projecting fibres, resembling short hairs or cilia. These hair-like fibres, though resembling in some respects cilia, differ from them in being much less sharply defined and less pointed; however, for convenience sake, I shall speak of them as cilia. Their direction is remarkable. At either extremity of the fusiform animal they are reflected backwards at a very acute angle, like the barbs of a feather, their direction being of course opposite at the two ends. They become less and less inclined as they approach the middle of the body, where they stand out at right angles to the surface. The apparatus of cilia-like processes above described is evi-

dently designed to give to the entozoon, whilst in this stage of its existence, the power of penetrating between the ultimate muscular fibrillæ, and thus to enable it to force its way from the interior of a primary fasciculus into the spaces between the muscular fibres. This will be the effect of the friction of the fibrillæ against the cilia, which will allow of motion in one direction only. And as its two ends must move in opposite directions, the cilia will also serve to aid the entozoon in its development longitudinally. That such is their office will be apparent on examining a sufficient number of specimens; in some of which the primary fasciculi will be seen to have been completely split up by these animals. But the correctness of this inference is more strikingly proved by the influence which the size and arrangement of the primary bundles of muscular fibres have upon the form and dimensions of the entozoa. Thus in the muscular parietes of the heart, where the primary fasciculi are smaller, and, from their frequent interlacing, shorter than in other parts, the cysticerci are, in this stage of their development, also very short and of a different form to those found in other muscles, composed of striped fibre, although in other respects perfectly similar; and, when completely formed, those taken from the heart cannot be distinguished from those formed in other muscles. The cells which have been alluded to as forming the principal part of the cysticercus thus far developed, and contained in the investment first described, are all of the same character, differing only in their form and size, according to their age and situation. Those situated about the centre, and forming the chief part of its bulk, are collected together into rounded masses, giving to many of the animalcules an obscurely annulose appearance. They are of an elliptical, or rather reniform figure. This form, however, is not essential to these cells, but merely results from the circular shape of the masses into which they enter, the convexity of each cell being a part of the outline of its respective mass. These cells contain minute granules, or rather molecules, which are variously disposed in different cells, so as to present a variety of appearances, such as circular spaces, which might be mistaken for nuclei, but which seem rather to be produced by a deficiency of the cell's contents at these parts, than by any distinct nucleus. The mode of formation of these cells must be examined in the growing parts of the animal, and for this purpose its

extreme ends are best adapted. When one of these ends is about to have an addition made to its length, the investing membrane at this part becomes at first very thin, and then disappears. A clear space is next seen, having in some specimens the form of the part which is about to be added to the extremity of the entozoon; in others it has no defined limit. This space contains, in some cases, nothing but extremely minute molecules, of different shapes; in others, these molecules are mixed with granules of various sizes, which have every appearance of having been produced by the coalescence of the molecules; and lastly, with these molecules and granules, there are in other examples very distinct globular cells, of a bright aspect, looking more like nuclei than perfect cells; these soon become flattened oval, and ultimately take the elliptical form before described. All the time these changes are taking place in the molecules and cells, the membrane has been in progress of formation, so that when the molecules have disappeared, and their place has become occupied by perfect cells, the end of the animal is completed. The cilia are soon afterwards added. The lateral growth of these animals takes place in the same manner: the first indication is a separation of the cilia, which, it must be observed, are larger at the sides of an entozoon than at the extreme ends; and then a thinning of the membrane supporting them; and, lastly, the formation of globular cells, as before noticed. After the animals have become of a considerable size, and forced their way from the interior of the primary fasciculi into the cellular spaces between the larger muscular fibres, they still continue to grow, especially in breadth; but they lose their cilia, and gradually acquire those parts which have been described as belonging to the neck. The first evidence of this addition is the appearance of inversion of the middle part of the cyst, forming a small hollow, the sides of which look as if thrown into folds containing granular matter, and the bottom presents a circular space in which are granular particles of various forms and sizes, but those in the centre are darker than the rest. It is from these particles that the suctorial disks, the hooklets, and the first of the laminated bodies are about to be formed, but as yet none of these parts are recognizable. At a stage a little more advanced, this apparent inversion of the cyst has increased, the neck has become longer, and the appearance of disks, hook-

lets, and laminated bodies is sufficiently distinct to be perfectly recognizable. The process of development is particularly apparent in the hooklets, and perhaps there is no other instance of the growth of an animal tissue which presents such facilities for the examination of the manner in which it is effected. First, because the part of the entozoon on which these organs are formed, is sufficiently transparent to admit of examination by the highest magnifying powers without any previous dissection. Secondly, because the material of which they are composed is so characteristic, and so dissimilar to the surrounding parts, that it can be detected in the minutest possible quantities. And, thirdly, as only a few of these hooklets are in progress of development at one time, and as these are in all stages of formation, every step in the progress of their growth can be traced from the merest molecule to a perfect hooklet. This is important in reference to the general theory of development, as it furnishes an example of the formation of a complete set of organs, on a plan more simple, and at variance with the cell-theory of Schwann and others. Before one of these hooklets takes on a recognizable form, it exists as a group of exceedingly refractive particles, all apparently of the same composition, and of a more or less globular form, but of very different sizes, some being so minute as scarcely to be visible by one-eighth of an inch lens, others being almost as large as the handle of a perfect hooklet, while the rest are of all dimensions between these extremes. The next condition of a hooklet is the apparent fusion or coalescence of some of these particles into the hooked part of the organ. Then the handle and tubercles are added, these having been previously formed by the fusion of the smaller particles, and these latter by the coalescence of the minutest and the minuter ones. Before the several parts are perfectly consolidated, their points of junction can be distinguished, and in other groups the fragments corresponding to those recently united can be recognized. Directly a hooklet is found, it is of its full dimension; and some of its parts are even larger and more clumsy-looking than in older hooklets. The substance of the particles entering into these organs, after they are once formed, undergoes no change in its microscopical characters, but is the same after as before their union. It is impossible to single out any one particle from the rest, which can be

taken for the nucleus of a cell, or for what physiologists would call a nucleated cell; and thus there is nothing which indicates that these organs have been formed by transformation of previously existing cells, but, on the contrary, there is every appearance that their formation is due to the simple coalescence of homogeneous molecules.

Up to the present point, the facts which I have stated are so obvious, that their accuracy will, I think, not be questioned; also the interpretation of them is not only that which appears to me the most natural, but is almost self-evident. There remain, however, some considerations of a more theoretical kind, though not of less importance. It will be asked, how the entozoon, in its earliest condition, such as I have described it, finds access to the interior of a primary fasciculus. Before attempting to answer this question, I must observe that my description commences from a condition of this entozoon so complete, that no one, on examining it in this state with the microscope, will deny its perfect similarity to those of the higher form. But there are other links in the chain which I must now consider, and which so far have been omitted only because I wished to keep that which is certain distinct from that which is probable. Before the cells and molecules already described accumulate in sufficient quantity to present the undoubted character above mentioned, they are found aggregated in smaller groups, and even occurring individually in all the primary fasciculi of the diseased muscle; their quantity, and the size and form of these groups, present the greatest possible irregularity in the different fasciculi. In some the molecular deposit looks like an early stage of fatty degeneration, but it has characters very different; one is the shape of the molecules, which resemble in all respects those in the growing ends of an entozoon; and another is, their situation, which seems to be between the primary fibrillæ, tending to separate them longitudinally; however that may be, it is an abnormal condition, and always co-existent with the higher forms of the cysticercus; and as the entozoon, as I have first described it, could not possibly have taken on that form all at once, these groups of molecules must therefore be looked upon as its antecedent stage, or as portions of cysticerci in progress of development. But I also find in the specimens of muscle infested with these entozoa,

many of the capillaries and smaller blood-vessels filled with organic molecules, which, so far as I am able to judge from the comparison of such extremely minute bodies, seem to resemble those molecules which are found in the primary fasciculi. The vessels filled with these molecules have their coats so thin as to be inappreciable, and some of the capillaries appear to be partially destroyed, and their molecular contents diffused among the sarcous elements. As this is an abnormal condition of the contents of these vessels, as well as of their coats, and, so far as my experience goes, is not found excepting in conjunction with the earliest stages of the cysticerci, I am inclined to believe that the molecules in question are the same as those in the primary fasciculi, and that it is by their coalescence in these fasciculi that the formation-cells of the cysticerci are formed.

Addition to the foregoing communication, received December 6, 1855.

After an entozoon has left the interior of a primary fasciculus, and arrived at the space between the muscular fibres, it loses its ciliated investment, and begins to increase in breadth. Its margin now seems to be formed entirely by the convexities of the globular masses of cells of which its body appears to be made up, causing it to present a crenate form similar to that of the ventral portion of the perfect animalcule, with this difference only, that these cells are compressed. The next change which is visible is the formation of folds, which become more perceptible as the animal increases in breadth, and which remain in the perfect entozoon so long as it is confined to a small space, but disappear when it gets to the space between the surface of a muscle and the fascia covering it. The unfolding in this last situation seems to be produced by the imbibition of fluid, and the consequent distension of the ventral part. These more advanced stages of the worm-form are best found in those specimens of diseased muscle in which the perfectly developed cysticerci abound. Their number in proportion to that of the perfect animalcules varies considerably in different specimens.

I have always succeeded in finding some of those of the worm-form along with the perfectly developed ones; and in some cases

there are as many of one kind as the other. After they have acquired a certain breadth—about one-twelfth, or the one-eighth of an inch,—the central part of the cyst appears to be drawn inwards, forming a hollow; at the bottom of which, the granular material is deposited from which the suckers, hooklets, and calcareous granules are formed, as above described.

December 20, 1855.

The LORD WROTTESLEY, President, in the Chair.

The President stated that Robert William Sievier, Esq., who by reason of non-payment of his annual contribution ceased to be a Fellow of the Society at the late Anniversary, had applied for re-admission; and an extract of his letter to the Council was read, explaining the circumstances under which, during his absence on the continent, the omission of payment had taken place. Notice was accordingly given, that the question of Mr. Sievier's re-admission would be put to the ballot at next meeting.

The following communication was read:—

"Further Researches on the Polarity of the Diamagnetic Force." By JOHN TYNDALL, Ph.D., F.R.S. Received November 27, 1855.

The author commences by referring to the results recorded in the Bakerian Lecture for 1855. The fact of diamagnetic polarity was there established, by permitting fixed magnets to act upon a moveable bar of bismuth, encircled by an electric current; and, from the deflections of the bar, the character of the force acting upon it was inferred. The experiments recorded in the present paper may be regarded as complementary to the above. Here diamagnetic bars, suitably excited, are permitted to act upon an astatic system of steel magnets, and from the deflections of the system, the polarity of the bodies acting upon it is inferred. An experiment of the nature here indicated was made, three years ago,

by Professor W. Weber; but, notwithstanding his known skill and accuracy as an experimenter, his results did not command general conviction. The author sketches the arguments that have been urged against the inferences which M. Weber has drawn from his experiments, and the conditions laid down by those who urged these arguments, for the rigorous demonstration of diamagnetic polarity. In the present paper these conditions are accepted and fulfilled.

To arrive at an exact notion of the value of M. Weber's experiments, the author thought it best to operate with an instrument similar to that used by M. Weber himself. He has to thank the latter philosopher for the plan of an apparatus more delicate than any which has been hitherto used, which plan was carried out in an efficient manner by that able mechanician, Leyser of Leipzig. The instrument consists essentially of two spirals of covered copper wire, about eighteen inches long, firmly attached to a massive slab of mahogany. The slab is attached by brass bolts to the solid masonry of the Royal Institution, so as to have the spirals in a vertical position. Above the spirals is a wooden wheel, with a grooved periphery, and below them a similar one. The wheels are united by an endless string, which communicates motion from one of them to the other. To this string the cylinders submitted to examination are attached; and by turning the lower wheel with a suitable key, the cylinders can be caused to move up and down within the spirals. Two steel bar magnets are arranged astatically, connected by a rigid brass junction, and so suspended that the magnets are in a horizontal plane. The two magnets have the two spirals between them, and have their poles opposite the centres of the spirals. When therefore a current is sent through the spirals, it exerts no more action upon the magnets than the central, or neutral point of a magnet would do. If the bars within the spirals be perfectly central, they also will present these neutral points to the suspended magnets, and hence exert no action upon them. But if the key be so turned that the two ends of the diamagnetic bars shall act upon the magnets, then, if these bars be polar, the intensity and character of their polarity will be indicated by the deflections of the magnets. Here, then, we have not only the action of the earth neutralized, but a turning force is brought to

bear upon the suspended system four times that which would come into play if only a single spiral and a single pole were made use of. The mode of observation is the same as that applied by Gauss to his magnetometer. The instrument is enclosed on all sides from external air-currents; the magnets have a mirror attached to them which moves as they move, and which is observed by means of a telescope and scale placed at a distance of about ten feet from the instrument.

When cylinders of bismuth are submitted to experiment, a very marked deflection is produced, indicating, on the part of the bismuth, a polarity opposed to that of iron. This is the only substance which has hitherto been examined; and against M. Weber's results, obtained with this metal, it has been urged that the deflection observed by him was due to induced currents, aroused in the bismuth by its mechanical motion up and down within the spiral. With regard to this objection, as bearing upon the author's experiments, he remarks, first, that the deflection produced is *permanent*, which could not be the case if the effect were due to induced currents, which vanish instantaneously. Secondly, if the effect were due to induction, it would be shown in the most exalted degree by the best conductors. Now antimony is less diamagnetic than bismuth, but it is a better conductor. The deflection produced by it, however, shows that it is its *diamagnetic* quality, and not its *conductive* quality, which is effective; the deflection is less than that of bismuth. Copper is fifty times a better conductor than bismuth, but its diamagnetic capacity is nearly nil; it produces no sensible action upon the magnets, which could not possibly be the case were the result due to induction.

Again, a quantity of bismuth was powdered, and the powder suffered to become so tarnished that it was unable to conduct voltaic electricity. Tubes filled with this powder produced effects almost as striking as those produced by the massive cylinders of bismuth.

But the experiments have been extended to a great number of insulators, with the same result. Heavy-glass, sulphur, calcareous spar, statuary marble, nitre, phosphorus, wax, and other insulators, have been examined, and proved polar. Both paramagnetic and diamagnetic liquids have also been embraced in the examina-

tion, and the polarity of both established. Thus every objection that has been raised against the polarity of the diamagnetic force has been removed, and an amount of evidence accumulated in its favour, which places it amongst the most firmly established truths of science.

The Society then adjourned over the Christmas recess, to the 10th of January, 1856.

November 30, 1855.

ANNIVERSARY MEETING.

The LORD WROTTESLEY, President, in the Chair.

Mr. Cayley reported, on the part of the Auditors of the Treasurer's Accounts, that the total receipts during the last year, including a balance of £1043 19*s*. 9*d*. carried from the account of the preceding year, amounted to £3231 16*s*. 0*d*., and that the total payments in the same period, including £2000 invested in the Funds, amounted to £4531 5*s*. 5*d*., leaving a balance due to the Treasurer of £255 9*s*. 8*d*.

The thanks of the Society were voted to the Treasurer and Auditors.

List of Fellows deceased since the last Anniversary.

Honorary.

His Imperial Majesty Nicholas Emperor of all the Russias.

On the Home List.

Martin Barry, M.D.
Sir Edward Ffrench Bromhead, Bart.
William Archibald Cadell, Esq.
Thomas Copeland, Esq.
Griffith Davies, Esq.
James Colyer Dawkins, Esq.
Sir Henry Thomas De la Beche.
Lewis Weston Dillwyn, Esq.
Bryan Donkin, Esq.
Right Hon. Henry Ellis.
George Bellas Greenough, Esq.
Wyndham Harding, Esq.
Joseph Hume, Esq., M.P.
The Right Hon. Sir Robert Harry Inglis, Bart.
James F. W. Johnston, Esq.
Henry Lawson, Esq.
Rear-Admiral Edward Lloyd.
Rev. John Maddy, D.D.
William Francis Spencer, Lord de Mauley.
Sir William Molesworth, Bart.
Rear-Admiral Sir William Edward Parry.
Joseph Phillimore, LL.D.
Philip Pusey, Esq., D.C.L.
William Rashleigh, Esq.
Rev. Richard Sheepshanks.
Edward Adolphus, Duke of Somerset.
Philip Henry, Earl Stanhope.
Percy Clinton Sydney, Viscount Strangford.
John Henry Vivian, Esq.
Thomas Weaver, Esq.
John Weyland, Esq.
John, Lord Wharncliffe.

On the Foreign List.

Karl Friedrich Gauss.
George Simon Ohm.

Withdrawn.

Captain Sir William Symonds, R.N.

Defaulters.

Robert William Sievier, Esq.
Rev. John Wright, M.A.

List of Fellows elected since the last Anniversary.

Arthur Connell, Esq.
Henry John Reynolds Moreton, Earl Ducie.
William Farr, Esq.
William Lewis Ferdinand Fischer, Esq.
Isaac Fletcher, Esq.
William John Hamilton, Esq.
Edward John Littleton Baron Hatherton.
John Hawkshaw, Esq.
John Hippisley, Esq.
James Luke, Esq.
A. Follett Osler, Esq.
Thomas Thomson, M.D.
Charles B. Vignoles, Esq.
Charles Vincent Walker, Esq.
Robert Wight, M.D.
Alexander William Williamson, Esq.
George Fergusson Wilson, Esq.

Readmitted.

Edward Blackett Beaumont, Esq.

The President then addressed the Society as follows :—

Gentlemen,

In addressing you for the first time, it is perhaps to be regretted that the course of events during the last year should have imposed on me the obligation of alluding to subjects which cannot be approached with too much caution, and which involve considerations of no ordinary difficulty.

One of the subjects may be described most generally as the "Relation of Science to the supreme authorities in the State;" and this is chiefly important as it affects the extent to which the claims of Science are recognized and regard paid to its interests by the ruling powers. For example, it has often been more than doubted whether the opinions of eminent men of science, even when expressed in the

most formal and deliberate manner, do, as things are now constituted, exercise sufficient influence over the deliberations of Government and Parliament when they are called upon to decide upon questions for the proper determination of which scientific knowledge must be considered as a valuable, and sometimes an indispensable qualification. Among the various causes to which this may be ascribed, it seemed to some that a want of representation of Science in the Legislature was possibly one; that is, that there was no one in either House of Parliament who was entitled to consider it his peculiar province to watch over the interests of Science and defend them, when, amidst the heterogeneous mass of subjects to which the attention of our Government and legislators is directed, it appeared probable that those interests would suffer, or receive less of that attention than their importance deserved.

With the view of remedying this defect, the Parliamentary Committee of the British Association for the Advancement of Science was formed; it consists of seven members of the House of Lords and six of the House of Commons, who are "requested to act as a permanent Committee to watch over the interests of Science, and to inspect the various measures from time to time introduced into Parliament likely to affect such interests."

My excellent predecessor in this Chair, who is himself a distinguished member of this Committee, thinks there is something in this arrangement "forced and awkward." It may be so, for it is necessarily a makeshift, a substitute for a more formal and recognized representation of Science in Parliament; but perhaps the more material question is, How has this arrangement hitherto worked? Has this Committee already done some good service to Science or not? The following is a short statement of what has been done.

1st. The Parliamentary Committee, in conjunction with your Council, favoured by the zealous cooperation of the present Chairman of the Board of Customs, succeeded in carrying out a very beneficial arrangement at the Custom House, by which the international communication of scientific publications has been greatly facilitated; and with the same view they have represented to the Postmaster-General the importance to science of prevailing on foreign countries to permit the extension to themselves of our excellent system of book-postage. A correspondence has been had with the Postmaster-

General and Mr. Rowland Hill, and interviews have been solicited and obtained. There is a great desire on the part of the Post Office authorities to carry out this object; and the only obstacle to success seems to be the reluctance of some foreign countries to consent to a perfectly free admission of publications.

2ndly. The subject of pensions to men of science has been brought before successive Governments, with the view of obtaining for Science its fair share in the distribution of the Parliamentary Grant, and impugning that principle of selection which implies that absolute poverty in the recipient ought to be an indispensable qualification for the receipt,—a principle of which the adoption appears to have originated in the smallness of the sum placed at the disposal of the Government for the recompense and encouragement of civil services.

3rdly. The Committee supported, and with success, a representation which had been already made to the Government by your Council, urging them to establish an office in this country to cooperate with Lieut. Maury in America, in collecting and reducing meteorological and hydrographical observations made at sea, and embodying the results in improved charts and sailing directions.

4thly. The Parliamentary Committee has supported the memorial addressed to Government, requesting them to appropriate some buildings in a central situation in this metropolis to the accommodation of the principal scientific societies.

5thly, and lastly. The Committee has, in a Report lately presented to the British Association at Glasgow, collected and embodied the opinions of eminent cultivators of Science on a question of great importance, viz. Whether any measures could be adopted by the Government or Parliament that would improve the position of Science or its cultivators in this country?

The absence of a representation in Parliament has been mentioned as being possibly one of the causes of the supposed want of deference to the interests of Science; but there are others which may well exercise some influence in bringing about a result, which, if its existence be admitted, must be a subject of regret to all whose opinions are deserving of respect.

It may be that there is something in the constitution of our Scientific Societies which may deprive them of their due weight and influence. In some foreign countries there are Institutes, or other

analogous bodies, recognized by their governments as part of the civil organization, and consulted on almost all occasions when scientific questions are under the consideration of executive or legislative bodies.

There seems ground for the belief, that any plan for the establishment of an Institute, properly so called, in this country, and certainly for organizing any direct dependence of Science on the Government, would meet with little favour; nor does there seem to be any desire that the aid now afforded by contributions from the public purse should be much increased; in truth, there is nothing that would inflict a more serious blow on Science and its cultivators, than that an idea should prevail that the latter had obtained from those entrusted with the conduct of public affairs a greater share of national pecuniary support than the well-considered interests of the community at large rendered desirable; there is nothing that ought to be more deprecated, for there is nothing that would be more fatal to the success of that which must always be our primary object, the object to which the interests of all societies, and of their members, must always be a secondary consideration, viz. the diffusion of Science itself, and the increase of our knowledge of the laws and phænomena of the universe.

At the same time, no one who has watched the course of recent events can fail to suspect that the influence of those who represent Science is rather less than greater than it ought to be. For example:—

Some applications for assistance addressed to the Government, and alluded to by your late President, were unsuccessful; and if disposed to speculate upon the cause of failure, we may possibly adopt the conclusion, that our Scientific Societies (not excluding even this, the most ancient and influential of all), owing to defects in their constitution or other causes, do not inspire the Government with sufficient confidence in their recommendations; and we adopt this conclusion the rather that we perceive no distaste for, or depreciation of, Science in the abstract among those whose influence is powerful in the State; on the contrary, we recognize among the members of successive administrations many known to be sincere friends to its advancement, and perceive also that great deference is sometimes paid to the individual opinions of men enjoying high scientific celebrity, and to those

of public bodies, such as the Board of Visitors of the Royal Observatory and the Trustees of the British Museum, which are invested with a more official character. Can it be possible then that the distrust of the recommendations of Scientific Societies as such, proceeds from some apprehension that in making them they are influenced by views of advantage to their own individual interests, which are opposed to the interests of the community at large? I should be reluctant so to think; but all will agree, that everything that can by possibility exercise any injurious influence deserves to be attentively considered and, if practicable, corrected.

Those who entertain that opinion will perhaps be disposed to receive with favour a scheme propounded for remedying the evil, which meets with the approval of some on whose judgment reliance may be placed; I mean the proposal to revive the late Board of Longitude under a new form, that is, to constitute a Board or Committee, composed partly of men holding high official situations, and partly of men of distinguished eminence in Science, to perform for its whole domain the functions which the late Board fulfilled for Navigation and Astronomy alone.

My distinguished predecessor in this Chair, while admitting the evils to which allusion has been made, has suggested a different remedy, that is, an augmentation in the number of your Council, and that the additional members shall be selected from such of our Fellows as, to use his own words, "have high general education, and are men of the world and of influence." It may well be doubted whether any change in the internal constitution of this Society, and least of all *that* change, will inspire the Government with greater confidence in its recommendations; in truth, the prestige of antiquity, and the belief in the scientific qualifications of your Council, are probably the chief grounds on which such confidence as is now extended to suggestions emanating from us is based; that confidence, therefore, might be still further impaired by changes in our constitution, and by admitting to our Council a larger proportion of men, who, however eminent in other departments of knowledge, have never made physical science their especial study or pursuit. However this may be, it cannot be otherwise than beneficial that these suggested remedies, which are now fairly before the scientific world, should be thoroughly canvassed and discussed in conjunction with any other

schemes which may be hereafter proposed for effecting a like object. It would be unreasonable to expect that any very general assent should be immediately given to any plan which has sufficient novelty to prevent its ready acceptance by some Fellows of this Society and others, who seem to have an instinctive dread of any Government interference with the independence of societies, and take a just pride in discharging, in this country, by their own gratuitous services, those functions which in other countries are performed by men appointed and paid by the State.

The views which have been put forward may seem to be in some degree confirmed by the recent correspondence and discussions respecting the £1000 entrusted to this Society for the promotion of scientific objects. It would be improper to go into any detail on a subject which has already been more than sufficiently discussed, and on some occasions in a spirit which was hardly called for or justified by the facts. Whilst the reasons assigned for withholding the grant seemed doubtless to imply a misapprehension as to the purpose for which it was originally conferred, and the part fulfilled by the Society in the administration of it, at the same time it was the duty of the Government to require a statement of the mode in which the public money had been expended, and also of the grounds on which a renewal of the grant was solicited; and it is to be regretted that any remarks should have been publicly made on the withholding of the grant until that information had been prepared and laid before the Executive, and they had had an opportunity afforded them of considering it, and deciding on their future course.

The Government, we may presume, are now satisfied with our distribution of the grant, and convinced of its utility, and they have decided in favour of its continuance. The whole matter has also been placed on a more satisfactory basis, by the resolution which has been adopted of making the grant for the future the subject of a special annual application to Parliament, to be made by the Government.

I allude however to this subject, because if it be not an example of misapprehension on the part of the Executive of that kind to which reference has been made, I would at least direct your attention to the account which has been recently published in our Proceedings of the manner in which the £5000 already granted by the Government has

been disposed of. You are aware that a Committee, consisting of about forty in number, and composed of men well known in science, have been the distributors of that fund; and I think that on carefully looking over the list of the various researches of interest to which it has been devoted, and weighing the paramount utility of the investigations and the value of the results, every one will join me in the expression of congratulation, that a grant so productive of advantage to the best interests of our country and mankind has not been withheld. At the same time it has been a subject of regret to the Committee of Recommendations, that either owing to the existence of the grant not having become generally known, or to some other cause, the number of applications of an eligible character to share in its distribution has not increased as much as might have been anticipated. It is possible that some modification of the stringent rules under which the money is now distributed may tend materially to enlarge the sphere of its usefulness. It is very much to be wished, that young men who have received a regular scientific education, and are anxious to devote themselves to researches for which their own pecuniary resources are inadequate, should be encouraged to avail themselves of this Government Grant.

I cannot take leave of the subject of the temporary withdrawal of this fund, without recalling to your memory the zeal with which Lord John Russell (to whom we owe the original proposal of the grant) and Lord Brougham then advocated the interests of Science, both unwearied promoters of the intellectual progress of the human race, and both have thus augmented their claims to the honours with which an impartial posterity will doubtless reward their services.

H.R.H. Prince Albert, on the occasion of laying the first stone of the Birmingham and Midland Institute, set forth the advantages of the study of Science in language calculated to make a powerful impression on the numerous auditory to which it was addressed. Lord Ashburton also showed, that, for want of a better scientific education in our artisans, we were likely to be outstripped in the race of commerce. May we view these events as examples of a growing desire to encourage scientific study and research, and diffuse their benefits more widely!

The question of the removal of the Society to Burlington House has received the anxious consideration of your officers. An inter-

view with Lord Palmerston, in the month of June last, had given us ground to hope that we should ere now have received some official notification of the intentions of Government on this subject. In this we have been unhappily disappointed; but all the information which has been received during the recess would lead to the conclusion that it has been determined to place Burlington House, or buildings erected on or near its site, at the disposition of the principal Scientific Societies of this Metropolis. No situation could be selected more favourable; and one cannot but anticipate that very important advantages will accrue to Science from their joint occupation of such a site. Among the most important of the benefits likely to flow from this change of our abode, I would specify,—1st, the facilities which it will afford to men engaged in common pursuits to meet and interchange their thoughts and views on subjects deeply interesting to all of them; 2ndly, the economical advantages that will result from a joint occupation; 3rdly, the benefits which will accrue to all the societies by the approximation of their libraries; and 4thly, the great increase of weight and dignity arising from so public a recognition of the claims of Science to national regard. At the same time we may purchase these advantages too dearly; and all the circumstances of the exchange must be well weighed before we finally agree to abandon our present location.

Since my acceptance of this office, I can truly say I have received the most unvaried kindness from all. The best understanding has always subsisted, and I trust will ever continue to subsist, between your officers; and the various matters of business which arise in the intervals between successive meetings of Council have been discussed by them at meetings held prior to each Council; by which arrangement, the officers bring to the Council a better knowledge of the principal subjects about to be deliberated upon.

In the Report of the Parliamentary Committee, to which allusion has been already made, various other matters of great importance have been mooted, which may be classed under two heads:—1st, those which respect the extension of scientific knowledge; and, 2ndly, those which have relation to rewards and encouragements to be bestowed on proficients in that department of learning. Under the first division, the most important questions, doubtless, that can be agitated are those of the means to be adopted for improving the

elementary instruction given in our schools, and for rendering the teaching of physical science more general at our Universities; but on these, time will not permit me now to enlarge. The difficult consideration of the extent to which public aid ought to be afforded to popular lectures, has been raised. I am not disposed to agree either with those who altogether condemn this mode of imparting instruction, or with those who anticipate great advantages as likely to accrue to the cultivation and diffusion of Science from its extension. There can be no doubt that latent talent has been sometimes called into existence by superficial teaching; and, on the other hand, that superficial teaching will never confer sound knowledge. Diligent and earnest private study alone can put the seal of authenticity on information acquired in the lecture-room. But when we consider what a large proportion of our fellow-subjects have neither the means nor the opportunity of studying at the Universities, or of otherwise acquiring the knowledge referred to, and the great advantages that would result to the middle classes and the higher grade of artisans, from acquaintance with at least the elementary truths of Science, it is worthy of serious consideration whether a certain amount of support by the State should not be conceded to popular lectures and also to educational establishments, at which the elements of the physical sciences may be taught on a more general and systematic plan to students, who shall be invited and expected to enter on their study with a serious intention of learning, so far as their means and opportunities extend.

In connexion with this subject, of the scientific instruction of the masses, it is impossible to overlook the effects which may be produced by the publication, within the last few years, of works written, it may be, in a somewhat unphilosophical spirit, and propounding theories which rest on unsubstantial foundations, but written with great ability, and calculated powerfully to excite the imagination of those by whom the truths of natural science have been little studied. Some students may perhaps require to have their attention aroused by the announcement of startling novelties, and these works may be to them the honey with which the bitter cup of abstract science must be anointed to attract their palates:—

> "Ut puerorum ætas improvida ludificetur,
> Labrorum tenus."

Under the second division of rewards and encouragements to those who are proficients in Science, the difficult subject of the utility of medals, decorations, and other stimulants of that kind, is discussed. As to medals, I agree with those who consider it expedient that some should be given; but care should be taken that such distinctions are not unduly multiplied. The subject of decorations, titles, and orders of merit, considered as incentives to the application to scientific research, is full of difficulty. On the one hand, it is doubtless an anomaly, in this country, that while these honours are now somewhat lavishly bestowed for military services, a very high degree of merit in any civil line is seldom rewarded by any such distinctions. On the other hand, it is clear that rewards which were not approved by public opinion would confer little honour, and that there are many eminent cultivators of Science in this country who would set little value on distinctions awarded by the Government, which would rarely be in a condition to form an accurate estimate of the claims of rival candidates.

That the emoluments of Professors should be increased, and more prizes provided at the Universities for scientific merit, is generally admitted.

There is one point on which the Report in question is silent, but that involves so serious a grievance, and is calculated so much to discourage the devotion of high mental endowments to physical research, that it would be improper to omit all mention of it here, when something like a cursory review of the present position of Science in this country has been attempted.

It has somehow or other become almost an established practice in this country, that no amount of labour, however arduous, performed for the benefit of the community by men of science, is considered, as such, entitled to pecuniary reward. I could enumerate many instances which have fallen within my own personal experience, in which a very great amount of anxious and harassing toil, wearisome alike to both body and mind, and calculated to exhaust the energies of both, has been performed for the State gratuitously by men of great eminence in their various walks of science, with a zeal and devotion worthy of all praise.

It is unnecessary, and it would be improper, to instance cases of services performed by persons now living; they are well known, and

some of them will occur to the great majority of my auditors; but there is one very remarkable and recent example, in which he to whom my observations relate is unhappily no more, and whose labours have been alluded to in our biographical notices,—I mean that of the late Mr. Sheepshanks, a name which I cannot mention without recalling, with affectionate regret, the various occasions on which he has kindly assisted with his advice and stimulated by his encouragement my humble labours in science *. As an original member of the Astronomical Society, and thus associated more or less with its Fellows for more than thirty-five years, I have witnessed the dawn, progress and final close of the honourable career of its most distinguished ornaments, many of whom I had the pleasure of numbering among my friends; and I can truly say, that men more disinterested, more ardent lovers of science and human progress, and more enlightened members of society, it was never my lot to meet; and when a more advanced stage of civilization shall be attained,—a period I fear long distant,—the labours of such men will be appreciated as they deserve to be, and they will be cited as bright examples of a life spent in fulfilling one of the noblest of tasks, that is, enhancing the dignity of man, by showing of what things the human mind is capable.

Your partial suffrages having placed me, however unworthy, to fill that distinguished post at the head of the most ancient and venerable of our British Scientific Institutions, I have thought it right to avail myself of this, the first opportunity afforded to me of addressing you, to take a review, necessarily hasty and imperfect, of some of the desiderata of Science; and having been so highly honoured above my deserts, I only desire that it may be said of me with truth, that I laboured diligently to induce my fellow-countrymen to regard with favour and respect, to cherish and foster, to appreciate and reward the labours of the cultivators of Science.

* Since the above was written, I have been informed that the late Sir Robert Peel offered £500 a year as a remuneration for scientific services in connexion with the restoration of the National Standards, and that both Mr. Baily and Mr. Sheepshanks preferred serving the nation gratuitously to the acceptance of what may have been considered an inadequate reward. The remarks in the text do not depend for their justification on a single example.

The Copley Medal for the present year has been awarded to M. Foucault, of Paris.

M. Foucault has been engaged in various remarkable researches during the last ten or twelve years. His earliest labours were devoted to photography. In the year 1844, he published, along with M. Fizeau, an investigation of the comparative intensity, both chemical and optical, of three of the most brilliant sources of light of which we can avail ourselves,—the sun, the voltaic arc, and incandescent lime*. The investigations of these philosophers led to numerical results, from which the vast inferiority of the lime-light came out in a very striking manner. While the voltaic arc with coke poles gave a light of which the intrinsic intensity was nearly $\frac{2}{5}$ths of that of the sun, the intensity of the oxy-hydrogen lime-light was found to be only about $\frac{1}{56}$th of that of the poles of the voltaic arc.

Shortly afterwards, M. Foucault was engaged, in conjunction with M. Fizeau, in a series of important researches on the interference of light produced by a considerable difference of path in the interfering streams†. In ordinary cases, all signs of interference cease when the difference of path amounts to a few undulations, though interferences of a high order had been observed with the light of a spirit-lamp with a salted wick, and also in certain peculiar phenomena exhibited in a pure spectrum. But the prism does not seem to have been employed in the investigation of interference, except for the sake of analysing the tints produced by means of polarized light; and theorists even doubted whether the vibrations of the body emitting the light in the first instance were sufficiently regular to render interference possible in the case of great differences of path. But by subjecting a narrow band of interfering light to prismatic analysis, the authors were able to detect perfectly distinct interference produced by great retardations, amounting in one case to no less than 7394 undulations, proving that the undulations are regular in a very high degree. A similar method of research enabled the authors to study the modifications of polarized light with great advantage.

* Annales de Chimie, tom. xi. p. 370.

† Annales de Chimie, tom. xxvi. (1849) p. 138, having been read to the Academy May 24, 1845; and Ann. de Ch. tom. xxx. (1850), p. 146, having been read March 9, 1846.

In September 1847, MM. Foucault and Fizeau read to the Academy an account of their researches on the interference of the calorific rays. By the use of very small spirit thermometers of great delicacy, they were enabled to detect alternations of intensity corresponding to those of illumination in the fringes produced by the mirrors of Fresnel. By the same means they detected alternations of temperature, corresponding to and coincident with those of illumination, in the interrupted interference-spectrum obtained by the methods employed in their last-mentioned researches; but the alternations of temperature were not confined to the region of the visible rays; they extended into the region of the invisible heat-rays of Sir William Herschel, situated beyond the extreme red. The authors established also the diffraction of heat, having succeeded in showing that the heat at a point a little outside the geometrical shadow of an opaque body with a straight edge, was greater than at such a distance from the shadow that the influence of the body was insensible.

On the 6th of May, 1850, M. Foucault communicated to the French Academy an account of a highly ingenious and striking experiment relating to the velocity of light*. The reflexion and refraction of light had long before been explained, both on the theory of emissions and on that of undulations. According to both theories, the velocity of light within a refracting medium is different from the velocity in air; but according to the theory of emissions it is greater, in the ratio of the refractive index to unity, while according to the theory of undulations it is less in the inverse ratio. The progress of optical science had since been such, that the theory of emissions was almost abandoned, while the theory of undulations continually gained fresh support from new phenomena. The effect of the interposition of a thin transparent plate in the path of one of two interfering streams, which had been deemed a crucial experiment, was wholly in favour of the theory of undulations. Still, the conclusion was only an optical inference from the observed result; the time of transit of the light did not intervene mechanically in the experiment. M. Arago had proposed to employ the revolving mirror of Mr. Wheatstone to decide the question mechanically, in a manner closely resembling that employed by Mr. Wheatstone in measuring

* Comptes Rendus, tom. xxx. (1850) p. 551, and Annales de Chimie, tom. xli. p. 129.

the velocity of electricity, and had shown by numerical calculation that with a high velocity of rotation the result would be sensible to observation. But the experiment proposed by M. Arago, though the execution of it might not have been impossible, would at any rate have been highly inconvenient for observation, because the result depended on the aspect of a momentary image appearing casually at an unknown instant in an unknown part of the field of view, and it was never carried out. The velocity of light had recently been measured directly for the first time by M. Fizeau; but in this case the observed time was that which light took to travel some miles in air. But by a highly ingenious contrivance, in which, by the introduction of a concave mirror, he was able to produce a fixed image of a revolving image, M. Foucault actually solved experimentally the question proposed for solution by M. Arago, though in a manner totally different from that suggested by that distinguished astronomer, and thus proved, for the first time by direct experiment, that light is propagated more quickly in air than in water.

On the 3rd of February, 1851, M. Foucault communicated to the French Academy* an experiment which has attracted more public attention than perhaps any other of modern times, namely his celebrated experiment of the pendulum. The phenomena of astronomy have long since proved the rotation of the earth; but we, living on it, are not sensible of that rotation. In one experiment only had the effect of the rotation been manifested, namely in the easterly deflection of a body let fall from a great height. But in this case the experiment could only be attempted in particular places, and the result was only a small deflection, which was liable to be interfered with by a variety of disturbing causes. But M. Foucault showed that a pendulum suspended so as to oscillate in all vertical planes alike, withdrawn from the vertical, and then left to itself, became by the very fact of its motion in some sense a celestial body; and while the plane of motion tended to remain parallel to itself, and would actually do so at either pole, the earth turned round under it, and thus the plane of motion was affected with an apparent rotation in the direction, in our hemisphere, of the hands of a watch, the velocity of rotation decreasing from one turn in twenty-four hours at the pole to zero at the equator, in proportion to the sine of the latitude,

* Comptes Rendus, tom. xxxii. p. 135.

and changing sign in passing into the southern hemisphere. This experiment, as is well known, had actually been carried out by M. Foucault, and has since been repeated by numbers of others.

More recently still, M. Foucault has invented another instrument, which he calls a gyroscope, for the experimental demonstration of the earth's rotation. The action of the instrument depends on the fixity of the plane of rotation of a disk made to revolve with great rapidity about its axis. The instrument is quite small, and may be used on a table, but requires great nicety of construction. By availing himself of the precessional motion of the axis when the disk is acted on by a force tending to turn it about an equatorial axis, M. Foucault is able, in consequence of the construction of the instrument, not only to exhibit the earth's rotation, but also to determine experimentally the plane of the meridian and the latitude of the place.

M. Foucault,

I present to you this Medal in testimony of our admiration of the skill, ingenuity, and talent displayed in your very remarkable experimental researches.

Your Council have awarded one of the Royal Medals for this year to Mr. John Russell Hind, Superintendent of the Nautical Almanac, for his researches and discoveries in Astronomy; an award, which I feel sure will meet the warm concurrence of the Society.

Mr. Hind commenced his astronomical labours as assistant to the Astronomer Royal, to which office he was appointed in November 1840.

Mr. Hind held that situation during four years, and was distinguished by his punctuality, attention and zeal; moreover, he devoted his leisure hours to the careful reading up of astronomy, both practical and theoretical; thereby familiarizing himself with the various methods of observation and reduction, as well as the calculation of orbits for comets and double stars. His first attempt at positive computation was an ephemeris of Bremicker's comet for 1840, which Mr. Airy printed in the Greenwich Observations of that year. In 1844, on being applied to by Mr. George Bishop,—a Fellow of this

Society, so well known for his attachment to science, and his munificence in furthering it,—the Astronomer Royal recommended Mr. Hind for the practical charge of that gentleman's private observatory in Regent's Park. It is for the observations made for Mr. Bishop, that the Council have made their award; and I beg to enumerate the principal.

Mr. Hind has discovered no fewer than ten new planets, and computed the elements of their orbits—first from his own observations, and again from those of other astronomers; and he has greatly improved our knowledge of the motions of the other members of the planetoidal group, by similar discussions, in each case, of all available data.

He has also discovered three new comets, and assisted greatly in procuring multiplied observations of them and of others, by his rapid calculation and speedy publication of successive approximations to the elements of their orbits; having thereby enabled many comets to be followed up through important portions of their paths, which would otherwise have been lost,—as witness the momentous and then unique case of his comet of 1847, which was observed at its perihelion passage at noonday, and in the immediate proximity of the sun, in consequence of the accuracy of Mr. Hind's computed places.

With the powerful and efficient means thus furnished by Mr. Bishop, this assiduous observer has, moreover, discovered two elliptical nebulæ, and a remarkable variable star in Ophiuchus, which, when first seen in April 1848, was of the fourth magnitude, and has now diminished to the twelfth. He has also noted the variability of other stars, including the very remarkable changes in S Cancri, of which he published an ephemeris. He has strengthened the evidence of the existence of a physical connexion between the constituents of binary stars; and—with Mr. Bishop—he has accurately mapped and published, for the advantage of astronomers in general, all the stars in a large part of the ecliptic region of the sky, down to and including the eleventh magnitude. These maps cannot fail to be of great utility in promoting the future discovery of planets and asteroids; in fact, of gleaning the heavens in that very interesting department. The dates and names of Mr. Hind's own discoveries in it being—

Names.	*Date of discovery.*
Iris	1847. August 13.
Flora	1847. October 18.
Victoria	1850. September 13.
Irene	1851. May 19.
Melpomene	1852. June 24.
Fortuna	1852. August 22.
Calliope	1852. November 16.
Thalia	1852. December 15.
Euterpe	1853. November 8.
Urania	1854. July 22.

Mr. Hind,

Accept this Medal, which the Royal Society presents to you, by an award which every astronomer will heartily confirm; and I may add our best wishes that length of years and renovated health may enable your enthusiastic zeal and high talents to render further aid to science, to discharge, with ever-augmenting fame, the duties of the responsible office which you occupy, and to reap additional honours for yourself and country.

Mr. Westwood,

I have the pleasure to inform you that the Council of the Royal Society have awarded to you one of the Royal Medals, on account of your valuable and long-continued researches in Entomology. The Council have been led to this decision from the fact that you have not confined your attention to any one region of the world, or to any one class of insects,—extensive as these classes are,—but have grappled with the whole subject. Besides a large amount of systematic work, you have particularly attended to the general principles of classification, and to the various kinds of affinity by which all organic beings are so curiously connected together; and in your study of different groups of insects you have carefully observed their habits, economy, metamorphoses and geographical distribution.

It is now nearly thirty years since you commenced the long series of papers which are well known to naturalists, not only of this country, but abroad; and that your writings have been appreciated on the Continent has been shown by their frequent translation.

Among your earlier labours, it is enough to refer to your well-

known 'Modern Classification of Insects,' in which the whole subject is treated in a masterly manner, as has been acknowledged by many most competent judges who have since profited by your labours. Of your more recent memoirs, which we have especially in view on this occasion, I may specify as particularly deserving of attention, those on the Cleridæ, Lucanidæ and Paussidæ, published in the Transactions of the Linnean, Entomological and Zoological Societies, your monographs on several Carabidous genera in Guérin's 'Revue Zoologique,' your contributions to Fossil Entomology in the Journal of the Geological Society, and the completion of the great work in folio commenced by Mr. Doubleday on the genera of the Diurnal Lepidoptera. Nor can I quite pass over your archæological researches, though these are not connected with the objects of this Society.

Mr. Westwood,—This Medal is presented to you in token of the interest which we take in your entomological researches, which reflect so much honour on your talents and zeal.

Obituary notices of deceased Fellows.

Dr. Martin Barry was born at Fratton, in Hampshire, on the 28th of March, 1802. Though originally designed for a mercantile career, his strong bent for scientific pursuits led to his embracing the medical profession, for which he studied in the Universities of Edinburgh, Paris, Erlangen, Heidelberg, and Berlin, as well as in some of the medical schools of London. He became a member of the Royal College of Surgeons of Edinburgh, and graduated as M.D. in the University of that city in 1833. During the period of his studentship he took an active part in the Medical, Royal Physical, and Wernerian Societies of Edinburgh, of which he was a member, and he was subsequently elected a Fellow of the Royal Society of Edinburgh. His love of fine natural scenery, and pursuit of botany and geology, led him to devote his college vacations usually to excursions in the mountain and lake districts of Scotland; and, after a session of study at Heidelberg in 1834, he spent the autumn of that year in a pedestrian tour through part of Switzerland.

In the course of these wanderings he arrived at Chamouni on the

15th of September, and, although it was then later in the season than any successful ascent of Mont Blanc had been achieved, Dr. Barry resolved upon the enterprise, and accomplished it with safety to himself and his guides. His was the sixteenth ascent that had been made: it occupied three days, owing to the unusual obstacles which the snow at that season presented; but the Doctor was rewarded by a magnificent view from the summit, and by weather so remarkably fine that "during the whole time he did not see a single cloud." In March 1836, Dr. Barry published an account of this ascent, which formed the subject of two Lectures delivered by him in Edinburgh, the proceeds of which he presented to the Royal Infirmary of that city. Baron Humboldt so highly esteemed the narrative and its author, that he personally requested Dr. Barry to translate from the German his (the Baron's) "Two attempts to ascend Chimborazo." This translation appeared in the Edinburgh New Philosophical Journal, 1837.

The difficult and recondite subject of animal development and embryology early attracted the young physician's attention, and he made himself intimately acquainted not only with the literature of that department of physiology, but with the eminent authors of the most valued works and treatises on those subjects. In the museums and laboratories of Professors Wagner, Purkinje, Valentin, and Schwann, Dr. Barry acquired that skill in microscopical investigations of which he subsequently made such excellent use.

He published a translation of the first part of 'Valentin's Manual of the History of Development,' in the Edinburgh Medical and Surgical Journal for 1836. From that period to 1840 he devoted himself exclusively to original researches on the development of the mammalian ovum and embryo, which at the time when he took up the subject was the darkest part of embryological science. The results of these researches were communicated to the Royal Society of London in three successive memoirs, entitled "Researches in Embryology." The 'First Series' was printed in the Philosophical Transactions for 1838; the 'Second Series' in the volume for 1839; the 'Third Series,' entitled "A Contribution to the Physiology of Cells," in that for 1840. In the same year Dr. Barry communicated a memoir "On the Corpuscles of the Blood," printed in the Philosophical Transactions for 1840. The Philosophical Transactions for 1841 contain his memoirs "On the Formation of the Chorion,"

"On the Chorda dorsalis," and two supplementary memoirs "On the Corpuscles of the Blood." The volume of the Transactions for 1842 contains his memoir "On Fibre," and that for 1843 his capital discovery of the "Spermatozoa found *within* the Ovum."

These Researches in Embryology contain a comprehensive, well-selected and well-conducted series of original experiments and observations on the formation and earlier stages of development of the ovum in the Rabbit and Dog, and in examples of the oviparous vertebrate classes from the bird to the fish. In the first series the author determined the order of formation of the different parts of the ovum, and the nature and mode of development of the vesicle called 'ovisac,' in which those processes take place. He made known the nature and traced the development of the so-called "disc of Von Baer," and detected in it a peculiar mechanism (retinacula) which he supposed mainly to regulate the transit of the ovum into the Fallopian tube. In the second series Dr. Barry traced the changes which the ovum undergoes in its passage through the Fallopian tube; the earliest and most interesting stages of mammalian development being for the first time described in this memoir. The important discovery of the segmentation of the yelk of the mammalian ovum is communicated in this memoir (1839), in which he first extended to that class the observation of a phenomenon which had previously been known only in the Batrachia. Dr. Barry's discovery made on the ovum of the Rabbit, was subsequently confirmed by Prof. Bischoff, in the ovum of the Dog and Guinea-pig.

Another most important observation, communicated by Dr. Barry to the Royal Society, in his Third Series, 1840, of the penetration by the spermatozoon of the ovum in the Rabbit, by an aperture in the zona pellucida, was of so minute and difficult a kind that it did not at once command assent. In 1843, Dr. Barry, however, published a confirmatory observation, which he had then made, of spermatozoa within the ovum of the Rabbit, taken from the Fallopian tube and in process of segmentation.

These statements at first met with positive denial by Professor Bischoff, who had failed in his attempts to repeat Dr. Barry's observations: and it was not until nine years afterwards that the observations of Dr. Nelson, on the impregnation of the *Ascaris mystax* *,

* Philosophical Transactions, 1851.

gave a corroboration of the fact of the penetration of the ovum by the spermatozoa. Mr. Newport next discovered the spermatozoa within the ovum of the Frog. Meissner soon after confirmed the fact that the spermatozoon penetrates the interior of the ovum of the Rabbit; and, finally, Prof. Bischoff satisfied himself of the truth of Dr. Barry's discovery, which he had been the first to call in question.

Dr. Martin Barry was elected a Fellow of the Royal Society of London on the 13th of February, 1840, and so highly were his communications esteemed by the Society, that the Royal Medal was awarded to him, November 30th, 1839.

Dr. Barry's embryological researches were followed by observations on the formation and changes of the primitive cells in the origin and growth of the tissues of the animal body, and he arrived at new and important conclusions on the importance and functions of the cell-nucleus.

In his memoir "On Fibre," Dr. Barry first promulgated his views as to the ultimate spiral structure of the muscular fibre; and he subsequently extended the same views to most of the tissues of organized bodies. These opinions have not been confirmed or accepted by histologists; although Dr. Barry had latterly the satisfaction of citing a few microscopical observations of eminent botanists which seemed to lend support to certain applications of his favourite idea. "Now that he is no more," writes an eminent physiologist, "it will be more pleasing to endeavour to extract that which was good and true in his works, rather than discuss their doubtful and contested points." And Professor Allen Thomson, in the same biographical sketch, writes, "It cannot be doubted that Dr. Barry's researches as a whole gave a decided impulse to the progress of knowledge in the departments of which they treat, partly by the actual contribution of new and valuable facts, and partly by the ingenuity of his speculative views, the vigour with which they were supported, and the discussions to which they gave rise."

Dr. Barry's latest contributions to science were chiefly notices and comments on the observations of other histologists and physiologists, which appeared to confirm or countenance his views of the cell-nucleus, the primitive fibre, and the penetration of the zona pellucida by the spermatozoon. "The last few months of his life were employed in a review of his microscopic observations, and in

forming, at the request of foreign physiologists, an abstract of them, to be published in Germany. Some portion of this work occupied his last hours, and he appeared to have a satisfaction in having done with it, as one leaving the world *."

The private circumstances of Dr. Martin Barry were such as enabled him to dispense with the pursuit of the practice of his profession as a means of support. The proportion of his time so saved was devoted to the poor, and chiefly in connexion with some public institution. In the year 1844, after the publication of his most original and important observations, and after receiving the high testimonial of the Royal Medal from the Royal Society of London, he accepted the office of House-surgeon to the Royal Maternity Hospital in Edinburgh; and Professor Simpson, the director of the institution, speaks of Dr. M. Barry as "our invaluable house-surgeon, and a gentleman to whose talents, zeal, and humanity the hospital is deeply indebted for its prosperity." Dr. M. Barry reared up diligent students, notwithstanding the harassing nature of the duties of a midwifery pupil; and his kindness, promptness, and unweariedness in rendering aid to the poor distressed parturient females in Edinburgh have made his name still gratefully remembered amongst them.

From 1849 to 1853, Dr. Barry's health, and especially his eyesight, having become affected by his close and persevering studies, he was induced to return to the continent, and resided successively at Göttingen, Giessen, Breslau, and Prague. At the latter city he resumed his microscopic studies of muscular fibre, with the cooperation of his friend Professor Purkinje; and the result of their combined researches is given in Müller's Archives for 1850, and in a translated abstract in the Philosophical Journal for August 1852.

In that year he revisited Scotland, and resided occasionally in Arran, Rothesay, and Edinburgh. His friends deeply grieved to witness, in his emaciated frame, the evidence of progressive malady. He suffered much from neuralgic pains, being deprived, therefrom, of rest for nights in succession. In the autumn of 1853 he finally took up his abode at Beccles, Suffolk, near his brother-in-law, Dr. Dashwood, who married Dr. Barry's only sister. Soothed and sustained by the devoted attention of his affectionate relatives, he

* Biographical Memoir in the Edin. Med. Journal.

lingered, with intellect unimpaired, and power of labour little abated, until the 27th of April, 1855. On that day, in the immediate prospect of death, he said, "All is peace;" then added, "even now;" and soon afterwards passed away, full of a Christian's hope.

Sir Henry Thomas De la Beche, C.B., F.R.S., F.G.S., Corr. Memb. of the Academy of Sciences of Paris, &c. &c., was born in London, February 10, 1796, married in 1818, received knighthood in 1842, was nominated C.B. in 1848, and died April 13, 1855. His education was conducted partly at home, partly at Keynsham and Ottery St. Mary, till in 1810 or 1811 he went to the Military College at Marlow. In 1817 he became F.G.S., and was admitted F.R.S. in 1819. From this epoch the prevalent bias of his mind toward Natural Science was manifested in a long series of valuable contributions to Geology, for the most part founded on personal research in districts to which he was ever partial, attached by early associations or allured by the instincts of an artist.

The southern coasts of England and Wales offered to the young and zealous student a series of interesting phenomena, at that time little explored,—rocks of sedimentary origin, exhibited in unusual circumstances; an uncommon variety of granites, greenstones, porphyries, and other rocks of fusion; singular complications of mineral veins, modern land-slips, ancient upheavings of strata, and undescribed organic remains. To all of these De la Beche brought a mind prepared; they became for him the main object of his observation and meditation; he returned to them again and again, fortified by the experience gathered in other parts of the world, and supported by the scientific alliance and strong personal regard of Buckland and Conybeare. Here was the centre of his field of inquiry, here his scientific life began, here he earned his fame, and it was while meditating and directing new labours in this favourite region, that he sunk to his long repose.

The following are some of his publications on the subjects alluded to:—

1819. The Rocks and Fossils of Devon. Geol. Trans.

1823. On the Geology of the South-East of England, from Bridport Harbour to Babbacombe Bay.

On the Discovery of an Elephant's Tusk near Charmouth. Geol. Trans. 2nd ser. i. 421.

1825. On the Geology of Southern Pembrokeshire. Geol. Trans. 2nd ser. ii. 1.

On the Lias of the Coast in the vicinity of Lyme Regis. Geol. Trans. 2nd ser. ii. 21.

On a Submarine Forest at Charmouth. Ann. Phil. xi. p. 143.

1826. On the Chalk and Sands beneath it in the vicinity of Lyme Regis and Beer. Geol. Trans. 2nd ser. ii. 109.

1827. On the Geology of Tor and Babbacombe Bays. Geol. Trans. 2nd ser. iii. 161.

1830. On the Geology of Weymouth (in conjunction with the Rev. Dr. Buckland). Geol. Trans. 2nd ser. iv. 1.

1834. On the Anthracite near Bideford. Proc. Geol. Soc. ii. 106.

1835. On the Trappean Rocks associated with the New Red Sandstone of Devonshire. Proc. Geol. Soc. ii. 196.

On Fossils from the Schistose Rocks of the North of Cornwall. Proc. Geol. Soc. ii. 225.

1836. Lettre sur la découverte d'Empreintes de Plantes dans les Schistes subordonnés de la Grauwacké. Bull. Géol. Soc. Fr. vi. 90.

1839. Report on the Geology of Cornwall, Devon, and West Somerset. 8vo.

1846. On the Formation of the Rocks of South Wales and South-Western England. Mem. Geol. Surv. i. p. 1.

On the Connexion between Geology and Agriculture in Cornwall, Devon, and West Somerset. Journ. Agr. Soc. Eng. iii. 21.

The natural history of the same region had other charms for the enterprising spirit of De la Beche. To dredge the sea, to gather the living wonders of the deep, suited the bold swimmer and skilful boatman; to examine the structure and habits of marine creatures was not less congenial to the microscopic observer and the accurate and forcible artist. The "Notes on the Habits of a Caryophyllia from Torquay" (Zool. Journ. 1828), and the "Catalogue of the Birds and Mollusca in the vicinity of Geneva," are indeed all that remain to mark the strong interest felt by De la Beche in recent Natural History; but they who have accompanied him over miles of land and sea know well the untiring delight with which, even in later life, he would scrutinize the isochronous movements of Rhizostoma,—the varying hues of Octopus,—the sensibility to light of the Bryozoa,—how sharp his attention to the peculiar instincts of the animal creation.

To such a mind came easily and naturally the inquiry into the osteological relations of the huge fossil reptilia, so long known, but so little understood by the collectors of fossils at Bath, Glastonbury

and Lyme Regis,—an inquiry which in the hands of Conybeare and De la Beche gave us the Plesiosaurus, that singular "link between Ichthyosaurus and Crocodile." (Geol. Trans. 1823, 1 ser. v.)

Love of scenery and ill-health induced him to prolonged residence abroad, and in 1819 and subsequent years we find him mapping and sounding the lake of Geneva—in 1823, tracing the geology of the north coast of France, examining the fossil plants of the Col de Balme—in 1824, 1825, exploring the geology of Jamaica, where lay his paternal estates—in 1828 and 1830, reporting on the geology of Nice and the Gulf of La Spezia.

In the midst of all his pleasant labour, De la Beche found time to prepare selections from the valuable memoirs in the Ann. des Mines (1824); 'A Tabular View of the Classification of Rocks' (1827); 'Geological Notes' (1830); 'Sections and Views of Geological Phenomena' (1830); 'A Geological Manual' (1831); 'Researches in Theoretical Geology' (1834); 'How to Observe' (1835); 'The Geological Observer' (1851). He officiated as Secretary of the Geological Society (1831), as Foreign Secretary from 1835 to 1846, and as President, 1848 and 1849. He was appointed Corresponding Member of the Academy of Sciences in 1853.

But the most important results of the labours of this eminent observer are contained in those valuable Geological Maps of the British Isles, which have been prepared partly by his hands, but entirely by his direction.

That which had been vainly solicited by the interests of agriculture in 1805, was conceded to the urgency of geology in 1832: the 'Ordnance Geological Survey' was begun, with De la Beche for its head or rather only officer, in the mining districts of Devon and Cornwall. From this epoch began a new era of British Geology, characterized by a minuteness of field surveying previously unknown, by exact measurements of the thickness and inclination of strata, by published maps and sections of unequalled truth and beauty. De la Beche's maps of the great western district,—one of the most difficult tracts in Britain for the geological surveyor,—appeared with a valuable explanatory report in 1839. In the map which accompanies the volume, the older strata of North and South Devon are called 'Grauwacke.' Their true relation to the old red sandstone groups, suggested by Lonsdale, Murchison, and Sedgwick,

was adopted in 1840 by the Director of the Survey, and made the subject of a separate volume drawn up by one of his friends.

From Cornwall and Devon the 'Ordnance Geological Survey' was transferred to South Wales, and before the close of 1841, the Director, with his staff of geologists, had measured the Palæozoic strata in all the cliffs of Pembrokeshire, and constructed maps extending from St. Bride's Bay to the sources of the Usk. In this large area the problem of the succession of strata has a different aspect from that which is presented in the eastern and northern parts of the Principality, and the distribution of ancient life offers many points for inquiry. These phenomena were discussed in the early memoirs of the Geological Survey, after an opportunity had been afforded of comparing them in detail with the typical Silurian tracts of Malvern and Ludlow, rendered famous by the earlier labours of Murchison.

Those who at this time shared the society of Sir Henry De la Beche in the field, experienced an enjoyment of no common description. Ill-health, the cares of office, anxieties of every kind, were swept away by the mountain wind, or forgotten amidst the glancing waves; every day brought new facts to an indefatigable observer, new scenes of beauty to an enthusiast in art, new occasions for profound reflection, sagacious inference, and practical instruction to his young companions. In after-years the Survey became too extended to admit of the same personal superintendence in every part. Separated from the 'Ordnance Survey' in 1845, it assumed the shape of a department, received local Directors for England and Ireland, an augmented staff, laboratory, lecture-room, and museum in London.

Over all this large establishment, the realization of his own plan, Sir H. T. De la Beche presided with the unflagging resolution which had brought it into being,—presided, indeed, too long. The strength which grew under the hammer, and rejoiced in long days of wandering over rocky hills, faded away among the official niceties and impediments of his great office; but he clung to his self-destroying work, and had before his death the satisfaction of seeing in full operation that Mining School, that Palæontological Museum, that systematic field geology, and professional teaching in practical science which he had kept steadily in prospect for twenty years.

In Sir Henry De la Beche, an active, prudent, and successful administrator has been withdrawn from the public service; but a far heavier loss is deplored in that branch of science in which he won his renown, and whose foremost cultivators were his friends, fellow-labourers, and disciples.

Mr. Bryan Donkin was born at Sandoe, Northumberland, on the 22nd of March, 1768. His taste for science and mechanics soon showed itself; and he was, almost as a child, continually to be found in his little workshop, making thermometers and ingenious contrivances connected with machinery of all kinds. This mechanical turn of mind was ultimately encouraged by his father, who was agent for the Errington and other estates, and who had formed the acquaintance of John Smeaton, the eminent engineer, from having frequent occasion to consult him on questions relating to the bridges and other works on the Tyne.

On leaving home, the son began life in the same business as his father, being engaged for a year or two as Land Agent to the Duke of Dorset at Knowle Park, Kent. Soon, however, the bent of his genius showed itself, by his leaving the Duke's agency, and going to consult Mr. Smeaton as to the best course to pursue to become an engineer. By Smeaton's recommendation, he apprenticed himself to Mr. Hall, of Dartford, and was soon able to take an active part in Mr. Hall's works; so that, in 1801–2, he was entrusted principally with the construction of a model of the first machine for making paper, the execution of which had been put into Mr. Hall's hands by the Messrs. Fourdrinier.

The idea of this machine originated with Mr. Roberts, and formed the subject of a patent obtained by Mr. Gamble, which was assigned to Messrs. Bloxam and Fourdrinier. After some time had been spent and considerable expense incurred, many attempts were made to set the model to work, but in none of these trials was any paper produced fit for sale.

The model remained at Mr. Hall's works until 1802, when Mr. Donkin agreed with Messrs. Bloxam and Fourdrinier to take the matter in hand; and, having taken premises at Bermondsey (still occupied by his sons), he made a machine, and erected it, in 1804, at Frogmore, Herts. On putting this machine to work, it was found

successful, but yet far from perfect. A second machine was made by Mr. Donkin, and erected, in 1805, at Twowaters, Herts, in which he introduced further improvements, although much still remained to be done. However, in 1810, eighteen of these complex machines had been erected at various mills, some of which are even now at work; and, at this period, having overcome the practical difficulties, Mr. Donkin erected in this, and various foreign countries, many similar machines, which rapidly superseded the method of making paper by hand. Thus for eight years Mr. Donkin gave his time and skill almost wholly to this one object; and his perseverance was crowned with signal success; for, although the original idea was not his, the credit of its entire practical development is due to Mr. Donkin.

The paper machine, of which at this time about two hundred have been made and erected by Mr. Donkin and his sons, ranks amongst the most useful and complete of mechanical contrivances; carrying the process uninterruptedly from the liquid pulp to the perfect sheet of paper, ready for writing or printing. The merit of these and of the later improvements introduced by the Messrs. Donkin was recognized by the award of the Council Medal at the Great Exhibition of 1851.

Mr. Donkin was also one of the earliest to introduce improvements in printing machinery. In 1813, he, in conjunction with Mr. Bacon, secured a patent for his Polygonal printing machine; and one was erected for the Cambridge University. It was then also he invented and first used the composition printing-rollers, by which some of the greatest difficulties hitherto experienced in printing by machines were overcome.

Mr. Kœnig and Mr. Cowper both used these rollers in their patent printing-machines, with Mr. Donkin's permission, which must be considered an act of the greatest liberality, since without these rollers no such machine can work. With the Polygonal machine, from 800 to 1000 impressions were produced per hour; but it never came into extensive use, as the construction was expensive, while the work produced was of a quality beyond that required in machine printing.

Mr. Donkin was also much engaged with Sir William Congreve, in 1820, in contriving a method of printing stamps in two colours, with compound plates, for the prevention of forgery; and, with the

aid of Mr. Wilks, who was then his partner, he produced the beautiful machine now used at the Excise and Stamp Offices, and by the East India Company at Calcutta.

Amongst the many inventions and ingenious processes in the promotion of which Mr. Donkin materially assisted, was the method of preserving meats and vegetables in air-tight cases. His attention was called to this subject in the year 1812, when he established a considerable manufactory for this purpose in Bermondsey. The introduction of this process has been of great public benefit; and on long sea voyages meat preserved in this way has become a necessary part of the stores of every well-appointed vessel.

Mr. Donkin was an early member of the Society of Arts, of which he was one of the Vice-Presidents; and as Chairman of the Committee of Mechanics, an office he held for many years, the soundness of his judgment and the urbanity of his manners made him much esteemed and beloved. He received two gold medals from the Society; one for his invention of an instrument to measure the velocity of rotation of machinery, the other for his admirable counting engine.

Although our space will not allow us to notice the various other inventions and improvements in machinery due to Mr. Donkin, we cannot pass over in silence his exquisite dividing and screw-cutting engine.

Mr. Donkin was much engaged during the last forty years of his life as a civil engineer, and was one of the originators and a Vice-President of the Institution of Civil Engineers, which was founded by one of his pupils, Mr. Henry Palmer, with a few other gentlemen; and Mr. Telford with Mr. Donkin obtained the Royal Charter for that body. In 1838 he was elected a Fellow of the Royal Society, and repeatedly served on the Council. He was also a member of the Royal Astronomical Society, and was held in such esteem by that body, that they placed him in the Chair on the occasion of receiving their Charter. He had, moreover, a small observatory in his garden, where he spent much of his leisure time; and it was to his own transit that he first applied his novel and beautiful level.

For many years Mr. Donkin was a magistrate for the county of Surrey, and, up to within a short time of his death, was very

regular and assiduous in the discharge of his duties. His life was one uninterrupted course of usefulness and good purpose; and he died on the 27th of February, 1855, after enjoying that general esteem and respect which render old age serene and happy.

It is now more than sixty years since CHARLES FREDERIC GAUSS, a young student resident in the city of Brunswick, hit upon an important theorem in the theory of numbers. His father was a bricklayer in very humble circumstances, who was anxious that his son should follow his own occupation; but the extraordinary capacity of the boy, at that time attending the National School, had attracted the attention of Bartels, afterwards Professor at Dorpat and the father-in-law of the great Astronomer Struve; and it was upon his recommendation that the reigning Duke, in spite of the opposition of the father, provided him with the means of a good classical education, by sending him to the Collegium Carolinum, and by many subsequent acts of kindness and patronage. The proposition which he had discovered, appeared to its author to be one of no ordinary beauty; and as he conjectured that it was connected with others of still greater value and generality, he applied—as he himself assures us—all the powers of his mind to find out the principles upon which it rested and to establish its truth by a rigid demonstration. Having fully succeeded in this object, he felt himself so completely fascinated by this class of researches, that he found it impossible to abandon them, and he was thus conducted from one truth to another, until he had finished the greatest part of the first and most original, if not the greatest, of his works, before he had read the writings of any of his precursors in this department of science, more especially those of Euler and La Grange. The subsequent study of the arithmetical researches of these great masters of analysis could hardly fail to expose him to the mortification which young men of premature and creative genius have so often experienced, of finding that they have been anticipated in some of their finest speculations. So far, however, from being repelled by this discovery, "I became," says he, "animated with fresh ardour, and by treading in their footsteps, I felt fortified in my resolution to push forward the boundaries of this wide department of science." The crowning result of his labours was, as is well known, the complete solution of binomial equations, and a most unexpected

extension of the limits within which the geometrical division of the circle had hitherto been confined; a discovery sufficiently memorable to form a great epoch in the history of the progress of geometry and analysis, and to place its author, in the estimation of the few persons who would appreciate its value, in the highest rank of the mathematicians of his age.

In dedicating his 'Disquisitiones Arithmeticæ' to the Duke of Brunswick, he acknowledges in very touching terms the wise and liberal patronage which had not only provided for the expenses of publishing his work, but also enabled him to exchange permanently the humble pursuits of trade for those of science. The work itself, as its author assures us, assumed many changes of form in its progress to maturity, as new views presented themselves from time to time to his mind; but, as is well known, the course which is followed in the invention of new truths is rarely that which is most favourable to clearness in their exposition, more especially when it has been pursued in solitude, with little communication with other minds; whilst the peculiar terminology which he has employed in the classification of numbers and their relations, and which is so completely embodied in the enunciation and demonstration of nearly every proposition that it can never be absent from the mind of the reader, renders the study of this work so laborious and embarrassing, that few persons have ever mastered its contents. Even Legendre, who had written so much and so successfully on the same subject, and who, in the second edition of his 'Théorie des Nombres,' makes the great discovery which this work contains the occasion not merely of special investigation but of the most emphatic praise, complains of the great difficulty of adapting its forms of exposition to his own; whilst the writers of the 'Biographie des Contemporains,' in a notice of the author at a much later period, when he had established many other and almost equally unquestionable claims to immortality, quote an extract from a Report of a Commission of the Institute of France, to whom it was referred in 1810, in which it is said, "that it was impossible for them to give an idea of this work, inasmuch as everything in it is new, and surpasses our comprehension even in its language." The biographers then proceed to stigmatize the book as full of puerilities, and refer to the success which it had obtained, including its translation into two languages, as affording grounds

for the presumption that "charlatanism sometimes extended even to the domain of the mathematics."

The first day of the present century was signalized by the discovery of the planet Ceres at Palermo, and before the first observations of the discoverer—only two in number—had been made known to astronomers, the planet had ceased to be observable from her proximity to the sun. The planet Uranus had been discovered twenty years before, when near opposition; this was a critical position, which at once gave a near approximation to the elements of his orbit: a stationary elongation of Ceres, though less fertile in its results, was sufficient to assign her such a place between Mars and Jupiter as was required to satisfy Bode's singular law, the recent announcement of which had already stimulated an enthusiastic band of German astronomers to commence a systematic search for the planet, which Kepler had found wanting for the fulfilment of one of that series of cosmical speculations which had guided him to the discovery of his laws. The complete determination, however, of the elements of a planet's orbit from three geocentric longitudes and latitudes—or from four of the first and two of the second in those cases where the latitudes are evanescent or small—was still therefore a new problem which had only been completely solved in the case of comets moving in parabolic orbits, and which Newton, to whom its first solution was due, had pronounced to be *problema omnium longe difficillimum*.

It was not until the month of October following the discovery of Ceres, that Gauss came into possession of the requisite observations, and in the course of a few weeks he had determined the elements of her orbit with an accuracy fully commensurate with the observations; so much so, indeed, that the Baron de Zach was enabled to rediscover the planet at the very first attempt which he made for that purpose on the 7th of December following. The elements of Pallas, Juno, and Vesta, the discovery of which followed that of Ceres at no great distance of time, were promptly determined by methods substantially the same, but materially improved by new artifices and adaptations of formulæ which an enlarged study and application had enabled him to give them.

The "Theoria motuum corporum cœlestium in conicis sectionibus circa solem ambientium," which contains not only the exposition of these methods and their detailed exemplifications, but a most elabo-

rate discussion of the various problems which present themselves in the determination of the movements of planets and comets from observations made on them under any circumstances, was not published before 1809. Gauss was not usually very prompt in making public his researches, but retained them in his own hands until, by repeated correction and examination, they had assumed a form which satisfied his own judgment of what was equally due to the requirements of science and his own honour; and the work of which we are now speaking exhibits, in a very remarkable degree, the effects of this severe system of revision, in the skilful adaptation and reduction of methods and formulæ, and in the careful estimate of the circumstances under which they may be most advantageously employed. We find in it no evasion of difficulties, and no resort to methods of approximation only, when the means of accurate determination are at hand. His aim was in every instance to obtain results of the same order of correctness with the observations upon which they were founded; and with a view of securing the full benefit of observations which furnish, as is usual in astronomy, data more numerous than the unknown elements which they are required to determine, he has given in the work which we are now considering the first completely developed theory of the method of *least squares,* more especially as applicable to astronomy, and of the means of estimating the degree or measure of precision which its application affords; and though he was anticipated in the publication of this method by Legendre, there is every reason to believe that it was with him an original discovery; for he is said to have been in possession of it as early as 1795. No other work in later times has contributed so much as this to the complete and scientific discussion of astronomical observations; and its influence is traceable in the form which those discussions have assumed in the writings of Bessel, Hansen, Struve, Encke, and other eminent astronomers, which have done so much honour to Germany.

It would be impossible in the brief space allowed for this notice to pass in review Gauss's various essays on subjects of pure and applied mathematics—some of them of great importance—which were generally communicated to the Royal Society of Göttingen, though most of them were separately published: amongst them we find two demonstrations of the resolvability of equations with rational terms into simple or quadratic factors; others on magic squares; on qua-

dratic residuals ; on a new method of determining integrals by approximation founded on Newton's method for that purpose ; on the theory of curve surfaces ; on the theory of capillary attraction, and on various subjects in dioptrics and astronomy : there is one memoir of more than common interest, devoted to the demonstration of a very remarkable proposition in the planetary theory, which is, that the secular variations which the elements of the orbit of a planet would experience from another planet which disturbs it, are the same as if the mass of the disturbing planet were distributed into an elliptic ring coincident with its orbit, in such a manner that equal masses of the ring would correspond to portions of the orbit described in equal times.

It was in the course of this last investigation that he arrived at some elliptic integrals, the evaluation of which he was enabled to effect by means of a transformation which is included in one of the series of transformations, the discovery of which will immortalize the name of Jacobi. It has been said—though we do not vouch for the truth of the anecdote—that this distinguished analyst was induced by his knowledge of this fact to seek—after his own discoveries were completed—an interview with the great mathematician who had thus intruded, prematurely as it were, into one of the deepest recesses of his own province : Jacobi submitted his various theorems to his inspection, and was met, as they successively appeared, by others of corresponding character and import produced from his manuscript stores, concluding with an intimation that there were still many more in reserve. Such an anticipation of discoveries, which totally changed the aspect of this difficult department of analysis, even if it had been as complete as it is here represented to have been, would have been no derogation of the rights which Jacobi has undeniably secured by priority of publication; but the wide circulation which has been given to this story, as well as our own knowledge of Gauss's habitual delay in the publication of his researches, have tended not a little to increase our anxiety to be put in possession of the various scientific treasures which he is said to have left behind him. It is to Lejeune Dirichlet that this task has been entrusted, and there are few living analysts so likely to perform it satisfactorily.

We now enter upon the last, and perhaps the most considerable

of Gauss's researches, which are contained in his various essays, both theoretical and practical, on the magnetism of the earth.

Of the three magnetic elements, the declination, the dip, and the intensity, the two first were formerly the almost exclusive objects of observation, though the methods which were employed for that purpose were generally too rude for the requirements of accurate science; but the third, or magnetic intensity, of which no use had been made in the business of navigation, was entirely neglected.

Humboldt first called the attention of philosophers to the great theoretical importance of this element, and he omitted no opportunity, in the course of his travels, of determining its value.

It was during his Arctic voyages that the attention of Colonel Sabine had been forcibly called to the consideration of this subject by the remarkable magnetical phenomena observed when approaching the magnetic pole; and it was principally due to his influence and example, and to the labours of Hansteen, Erman, and other eminent travellers and navigators, that observations of the intensity were rapidly multiplied in every part of the globe, and more especially in Siberia, which had been generally believed to be the site of a second northern magnetic pole. These observations, 753 in number, in 670 different localities, were collected, arranged, and discussed in an admirable report which was made by Colonel Sabine to the British Association in 1837; and it is not one of the least of the many claims of its author upon the gratitude of men of science, more especially in connexion with magnetic researches, that it not only suggested to Gauss—as he himself declares—his bold attempt to grapple with the general theory of terrestrial magnetism, but furnished him with the materials for testing the applicability at least, if not for establishing the truth, of the theory which he proposed.

The observations of the terrestrial intensity which had hitherto been made were comparative only, and it was with a view of converting such comparative into absolute measures, with reference to determinate units, that Gauss undertook the series of investigations which are recorded in his memoir, entitled "Intensitas vis magneticæ terrestris ad mensuram absolutam revocata," which was published in 1832. He was assisted in these experiments, as in all others that he made, by Weber, a philosopher who is well known by his "Wellen-

lehre," written in conjunction with his brother, as well as by many other works of his own, but who felt that he was honoured by the privilege of combining his labours with those of so great a master. The units of reference which were chosen by Gauss were the millimetre in length, the milligramme in weight, and the second in time; and the horizontal intensity at Göttingen, in terms of these units, was found to be 1·7625, which gives, assuming a dip of 68° 1′, a total intensity represented by 4·7414.

It followed, as another consequence of this inquiry, and which may serve to give us a conception of the vast forces with which we have to deal, that the magnetism of the earth might be replaced in external space by the combined action of 8464 trillions of magnet bars, with parallel axes of the weight of one pouud each; or, if we should assume the magnetism of the earth to be uniformly distributed throughout its substance, the magnetism contained in four cubic feet of its matter would be nearly equivalent to one such magnet.

The publication of this memoir, a model of the union of experimental and theoretical research, produced no ordinary effect upon men of science, particularly in Germany. It was felt that the time had arrived when the same precision which had thus been found to be attainable in the absolute determination of one of the magnetic elements, would not only be equally so in the determination of the others, but also of the changes, whether periodical or occasional, which they were known or suspected to undergo. Were the disturbances of the needle, which had been observed to be produced at distant places by the aurora borealis, or other less manifest causes, absolutely simultaneous; or were they not so? A magnetical observatory, for the purpose of making the observations which these inquiries suggested, was established at Göttingen, under the superintendence of Gauss and Weber, by whom also instruments were designed which were capable of giving results incomparably more accurate than any which had hitherto been attained. Observatories on the same model were formed in various cities in Germany, and ultimately at Greenwich; the members also of a widely-spread magnetical association engaged themselves to make simultaneous observations on certain term-days and hours; and the fine series of magnetical observatories which were subsequently established at the Cape of Good Hope, Hobarton, Toronto, and elsewhere,

and furnished in almost every instance with a numerous and well-organized staff of observers, was the final triumph of a system which had originated at Göttingen, and which has already sufficiently pointed out the general laws, as well as the anomalies of magnetic action, though unhappily it has hitherto left the physical causes which give rise to them almost entirely untouched.

We have already referred to the circumstances which suggested the celebrated Memoir "On the Theory of the Earth's Magnetism," which was published in 1839. There is, properly speaking, only one known physical principle which can be assumed for its basis, which is the variation of the magnetic forces, according to the inverse square of their distances. It is this principle which brings into operation a function, named by later writers the *potential* function, which had been already extensively used by La Place and Poisson in some of their most difficult investigations arising out of the theory of gravitation. The differential coefficients of this function would express the coordinate components which determine the direction and intensity of the earth's magnetism, and provided they were known, they would assign the three elements which we are in search of; but inasmuch as the law of the distribution of magnetism within the earth is altogether unknown, so likewise is the form of its potential function, and the process of deduction of the conclusions which we are required to draw from it would thus appear to be stopped at its origin. Yet there are some general properties of the function itself, and some also which are deducible from the known conditions which it is required to satisfy, which have enabled this great master of his art, with singular sagacity and skill, to make even the dumb to speak, and to give responses which are of the highest philosophical import.

Such is the clear conception, which he educes from it, of the characteristic property of a magnetic pole, and the necessary consequence resulting from it, that there can be only one northern and one southern pole; and the consequent effectual dissipation of the conclusion which so many eminent philosophers had drawn from their observations, that there were two northern, and by a natural inference, therefore, two southern magnetic poles. Such also was the remarkable proposition, that if the component of the horizontal magnetic force directed towards the north was given for the whole

surface of the earth, then the horizontal component directed towards the east or west would follow of itself; and not less remarkable was the consequence deducible from this, that the knowledge of the value of the potential function which the horizontal component, as above stated, would furnish for all points of the earth's surface, would also give its value for all points of external space. But of all the conclusions which this memoir contained, those which excited the most sanguine hopes of the ultimate and complete solution of the great problem of the earth's magnetism, were the successive theorems in which he showed that the components of the magnetic force for any point of the earth's surface may be represented by combining, with given functions of the latitude and longitude of that point, certain constant coefficients—of which not more than twenty-four were likely to be required—which were deducible from a sufficient number of the observed values of those components in different and assigned localities.

The calculation of these coefficients, a work of no ordinary labour, was effected by the author of this theory; and the results which they afforded were compared with their values, as given by observation, at ninety-one stations. The discrepancies between observation and theory, which were shown by these results, were not more considerable than might have been expected from the inadequate extent to which the calculations had been carried, and from the necessarily imperfect character of the data which were made subservient to them. The calculation of the same coefficients was renewed, and greatly extended, by Petersen, under the direction of the younger Erman, at the request and expense of the British Association, and the results are published amongst their Reports for 1847. Some of these results would seem, however, rather to indicate defects in the theory than errors of the observations, and it seems highly desirable, with a view of further testing its correctness and applicability, that the calculations should be resumed with the aid of more accurate and multiplied observations.

In a subsequent memoir Gauss enters upon a discussion, which is at once elementary and profound, of the general properties of the same potential function which plays so important a part in the "Allgemeine Theorie des Erdmagnetismus," ending with a series of propositions on the relations of this function for a distribution of

magnetism—whether entirely upon the surface of the earth, or within it, or in external space—which seem to be the ultimate conclusions to which this theory has hitherto attained or is capable of attaining. This memoir presents a striking illustration of that happy union of analytical skill with philosophical power for which his later writings were so remarkable, and which puts them in striking contrast with the obscurity and extreme compression of some of his earlier productions.

Gauss was born on the 30th of April, 1777. After completing his education at the Collegium Carolinum, he proceeded to the University of Göttingen in 1795; he graduated at Helmstadt in 1799, and afterwards resided as a private teacher in Brunswick until the year 1807, when he was appointed Professor in the University of Göttingen, and Director of the Observatory; a situation which he continued to retain for the remainder of his life. He was twice married, and by his first wife, who died in 1809, he has left one surviving son, and by his second, two sons and a daughter, Theresa, to whom he was tenderly attached, who nursed him, when his health began to decline, with the greatest affection and care.

During the last year of his life the decay of old age began to manifest itself in a disease of the heart, and the usual symptoms which accompany it; and he died in great tranquillity on the morning of the 23rd of February last. "I assisted," writes the Baron von Waltershausen, one of the most distinguished of his pupils, in a letter communicating these facts, "with others of his friends and pupils, in placing his body in his coffin, in binding a laurel crown around his head, and in discharging with filial love and reverence the last honours of the great man, whose name is destined to take its place with those of Archimedes and Newton in the history of the exacter sciences." On the 26th of February, his body, which had lain in state in the Rotunda of the Observatory, was followed to the grave by the whole University, and by a vast multitude of friends and admirers.

George Simon Ohm was born on the 16th of March, 1787, at Erlangen in Bavaria, where his ancestors had been known as prosperous and skilful locksmiths for several generations. His father's intention was, that he, as well as his younger brother, Martin

Ohm, should learn the family craft; but having himself acquired an amount of knowledge—especially of mathematics—unusual in his station of life, which he had found useful to him in his business, he resolved that his boys should have the advantage of a superior education before entering on their future calling, and accordingly, after they had passed through the Elementary School, he sent them to the Gymnasium. With such opportunities and the example of their father, it is not to be wondered at that the talents of the two brothers were rapidly developed. A new career opened to them in 1804, when the celebrated mathematician Langsdorf having become acquainted with their extraordinary progress, pronounced the judgment that some day they would emulate the brothers Bernouilli. He prepared a certificate to this effect, which induced their father to relinquish his intention of bringing them up to his business, and to allow them thenceforward to pursue a scientific career.

George Simon Ohm entered the University of Erlangen when he had completed his sixteenth year, but he remained there only eighteen months, leaving it to give instructions in mathematics in Switzerland. In August 1806 he became a mathematical tutor in the Institute of Gottstadt near Nidau, in the Canton of Berne; after remaining here two years and a half, he went to Neufchatel, where he spent the next two years and a half as a teacher of mathematics. Towards the end of 1811 he returned to Erlangen, and, after taking his degree, entered on an academical course of life as a *Privatdocent* there. This position, however, was merely temporary, as well as a tutorship which he subsequently held at the "Realschule" of Bamberg, which was soon dissolved.

Ohm attained in 1817, for the first time, a suitable and permanent position as teacher of mathematics in the Great (Jesuits') Gymnasium at Cologne, where the peculiar faculty he possessed of representing the theory of mathematics in a comprehensive and attractive manner to the youthful understanding was soon recognized. Ohm, however, had an ambition higher than that of remaining a mere mathematical teacher; his genius led him on to travel into the less trodden regions of science, and to try his powers as an original inquirer. It was not long before he found a congenial sphere of action, and was led to discover the true explanation of the hitherto enigmatical phænomena of the voltaic current.

In 1826 he obtained a long leave of absence in order to proceed to Berlin to perfect and publish his new theory, which appeared in 1827 under the title "The Galvanic Circuit mathematically treated, by Dr. G. S. Ohm." When this work first appeared, it had not the fortune to attract notice from the leading scientific men of the day, nor did it gain for its author consideration or favour from the aurities then at the head of the affairs of education and learning in Prussia, by whom, indeed, he was received in a manner which showed an entire misapprehension of his scientific activity and of his great merits. His susceptibilities thus wounded, he did not delay a moment to declare that, after such a reception, it was impossible for him to retain the appointment he held at Cologne. With the deepest feelings of mortification and grief, he left the place, thrown back into private life with most precarious means of existence, and deprived of all the requisite resources for pursuing his investigations. Seven of the best years of his life were in this way lost to science; but from these adverse circumstances he was at last withdrawn in 1833, when the Bavarian government appointed him Professor in the Polytechnic School at Nuremberg.

Whilst Ohm was usefully employed in this new sphere, his theory of the voltaic circuit began to be appreciated both at home and abroad, and in 1841 the Royal Society of London awarded to him the Copley Medal, the highest honour in its power to bestow; and to mark still more its high estimation of the eminent services he had rendered to science, he was elected, in 1842, a Foreign Member of the Society. This judgment, it is acknowledged by his countrymen, had the effect of entirely removing the obstacles which had hitherto impeded his way; the conclusions of his theory became known as "Ohm's laws" in all elementary works on physics, and throughout Europe his position was recognized as among the most eminent philosophers of Germany.

Amidst his active duties as Rector of the Polytechnic School at Nuremberg, and Professor of Physics, he found time to make advances in the scientific career which his theory of the voltaic circuit had opened to him. Physicists have long been convinced that the various forces to which we ascribe the phenomena of Light, Heat, Electricity and Magnetism, must all have a common origin; transformations of one series of phenomena to another have even been

shown experimentally without the known facts having led to the discovery of their intimate connexion. To this important investigation Ohm determined to devote the remainder of his life. The peculiar views which he had adopted in his researches in electricity respecting the interior constitution of bodies and of the molecules of which they consist, appeared to him to throw a new light on the nature and co-relations of the forces referred to. Following out these ideas, he established the general properties, form, and arrangement of the molecules; he attributed to them simple and polar powers; he determined their relations to the various external actions, and thus gradually formed a complete system, from which he saw the phenomena of light, heat, electricity and magnetism evolve themselves.

Of this projected work on 'Molecular Physics,' only one volume has appeared, which was published in 1849 under the general title of "Contributions to Molecular Physics," vol. i., and with the special title of "Elements of Analytical Geometry of three dimensions according to the system of oblique-angled co-ordinates." This introduction he thought necessary, because the ordinary mathematical methods did not appear to him to apply themselves to his ideas with sufficient simplicity and conciseness. He dedicated this work to the Royal Society of London, "whose approbation," he says, "tempered his courage, which had previously been softened by disheartening treatment, to renewed efforts in the field of science."

Whilst Ohm was, with incessant industry, carrying out his great undertaking, he was, towards the end of the year 1849, unexpectedly called to fill the vacant place of Conservator of the Physical Collection at Munich. Agreeable as this appointment must have been to him, and in accordance with the new scientific direction he had taken, still the event is to be regretted, as the arrangement of the Museum and the construction of new instruments withdrew his attention from continuing and completing his great work. During this time he published a memoir of great interest on the phenomena of interference in uniaxal crystals.

In 1852 changes occurred which induced Ohm to relinquish the official position he had gained in Munich and to become Professor of Experimental Physics in the High School of that city. Not contented to restrict himself to the customary demonstrations and explanations, which would have cost him but little exertion, he pre-

pared for his lectures a text-book on Physics, in which many of the subjects were treated in a very original manner. This work was published in 1854.

To complete, within the limited time he saw before him, the labours he had undertaken, required unusual exertions, which his feeble constitution did not enable him to support. His friends remarked with regret the gradual sinking of his forces from the beginning of 1854; he however continued his lectures until a renewed attack of apoplexy suddenly terminated his life on the 7th of July, 1854. "Thus ended," says his biographer, "the noiseless life of a simple and easily contented, but highly gifted man, who lived solely for science, and who had neither sought nor found social advantages, honours, wealth, or what the world is accustomed to consider as chiefly contributing to happiness." About a year before his death, however, the Cross of the Order of Merit of St. Michael was conferred upon him, and he was also made a member of the newly-founded order of Maximilian.

Ohm was a man of small stature. His countenance, although usually earnest, expressed his good nature and modesty. He was little inclined to conversation, but what he spoke was the expression of his soul, always full of matter, and frequently enlivened with wit and sprightly humour. In his life and habits he was extremely simple, contented and temperate. He was fond of solitude, and to this feeling, as well as to the unfavourable circumstances with which he was surrounded at the commencement of his career, was it perhaps owing that he never sought to establish his domestic happiness by marriage.

Besides the works alluded to in this notice, he contributed twelve papers to the Journals of Schweigger and Poggendorff. Two of these relate to Acoustics, one to Physical Optics, and the remainder are on Electrical subjects. The latter consist principally of experimental verifications of his theory, some published before and some after the appearance of his mathematical treatise.

The particulars above stated are derived almost entirely from a detailed memoir on the life and writings of this eminent philosopher communicated to the Royal Academy of Sciences of Munich by Dr. J. Lamont.

Rear-Admiral Sir William Edward Parry (Knight) was the fourth son of the late Dr. Caleb Hillier Parry, F.R.S., an eminent physician of Bath, who married Miss Rigby, sister of the late Dr. Rigby of Norwich, and grand-daughter of Dr. Taylor, author of the Hebrew Concordance. He was born at Bath, Dec. 19th, 1790, and received his education at the Grammar School of that city. At the age of twelve he entered the Royal Navy under the patronage of Admiral Lord Cornwallis, who commanded the Channel Fleet, and had his flag flying in the 'Ville de Paris.'

Intelligent, active and ambitious, Parry soon introduced himself to notice, and we find the Admiral making this early mention of him in a letter to a friend. "It is a pity," he writes, "that Mr. Parry had not gone to sea sooner, for he will be fit for promotion long before his time is out." In 1806 Mr. Parry joined the 'Tribune,' Captain Thomas Baker, and subsequently the 'Vanguard' under the command of the same officer, with whom he served the remainder of his time as midshipman. On the 6th of January, 1810, he was promoted to the rank of lieutenant, and appointed to the 'Alexandria,' Captain Quilliam, employed in protecting the Spitzbergen Whale Fishery, and thus became first acquainted with that vast icy element with which in after years he was destined to contend. He subsequently served in the 'Hogue,' 'Maidstone,' and lastly the 'Niger,' Captain Samuel Jackson, C.B. While in the 'Hogue,' he accompanied a detachment of boats, and assisted in the destruction of twenty-seven of the enemy's vessels, three of which were heavy privateers. This and some sharp skirmishes with the gunboats of Denmark, are the only actions with the enemy that fell to the lot of the subject of our memoir, as the peace of 1815 happily put an end to all such exploits.

In 1817 the dangerous state of his father's health obliged him to proceed to England on leave, an event of a momentous character in the career of Parry, for it was at this period that Sir John Barrow brought to the notice of the Admiralty the extraordinary changes which had been reported to have occurred in the state of the Polar ice, and the remarkable advance to a high northern latitude that had been made by Captain Scoresby in a whale-ship, and urged upon the Government the project of renewing the attempts which had been formerly made to reach Behring's Strait by way of the

Polar sea. Hence the commencement of that series of northern voyages, which have so much redounded to the honour of this country, and which afforded to the subject of our memoir an opportunity for the exercise of those high qualifications for the conduct of an arduous and difficult undertaking, which so conspicuously marked his character.

The Admiralty having determined upon sending out two expeditions to the Arctic seas, one in the direction of Spitzbergen and the other through Baffin's Bay, Lieutenant Parry was selected for this service, and on the 14th of January, 1818, he was appointed to the command of the 'Alexander,' a hired vessel commissioned for the purpose of accompanying Captain, afterwards Sir John, Ross on a voyage for the discovery of a north-west passage by way of Davis's Strait.

The Expedition quitted England in April 1818, and although unsuccessful in the accomplishment of the great object of the undertaking, succeeded in circumnavigating Baffin's Bay, and in restoring to that arm of the sea its outline—which had been erased from our charts—nearly as it had been drawn by the early navigator whose honoured name it so deservedly commemorates.

The examination, however, of the various sounds which broke the outline of that inland sea had not been made by the Expedition with that care which was necessary to satisfy the inquiring spirit of Parry, more especially with regard to a wide opening in the coast, to which Baffin had assigned the name of Sir John Lancaster's Sound. This opening, which when discovered by our navigators had excited the brightest expectations, and from which Parry and his associates had been compelled to return with the utmost regret, Parry on his arrival in England found to his astonishment represented as closed by a lofty range of mountains bearing the name of the first Secretary of the Admiralty, Mr. Croker, barring all progress to further discovery in that direction.

The indignation of Parry at this extraordinary misrepresentation may be imagined, and he did not flinch from the responsibility of disclosing his sentiments to the Admiralty, who immediately determined upon sending another expedition to the same quarter to decide the question. Accordingly in the spring of 1819 the 'Hecla' and 'Griper' were fitted for the purpose, and the command entrusted to Lieu-

tenant Parry. The Expedition quitted England in May 1819, and reached Davis's Strait early in July. The progress of the ships being arrested by the ice, which at the early part of the summer was known to the whalers to occupy the upper part of Baffin's Bay, the character of Parry at once showed itself, by a prompt determination to attempt a passage through the opposing boundary. He accordingly dashed into the ice with both his ships with a boldness which deserved success, and accomplished a passage through this great barrier, till then considered impenetrable.

The Expedition reached Lancaster's Sound on the 1st of August, and found it entirely free from ice. "We were now," says Parry, "about to explore that great inlet which had obtained a celebrity beyond what it might otherwise have been considered entitled to possess, from the opposite opinions which had been held with regard to it, and it will be readily conceived how anxious we were to advance." His suspense was not of long duration, for in a very few days he had the high gratification to be able to clear up all doubt, by the advance of the ships over that imaginary chain of mountains which had been drawn across the Sound as if purposely to disarm inquiry, and by the discovery of a wide and magnificent strait opening out into the Polar sea, to which was given the name of Barrow, as a well-deserved compliment to the second Secretary of the Admiralty as the strenuous promoter of Arctic discovery.

Hour after hour rolled away, and the ships continued their uninterrupted progress upon a direct course for Behring's Strait. Who but Parry himself could know the feelings which filled his anxious mind at that time! His associates, as they witnessed the clear open sea rise in the horizon mile after mile, might be elated at the brilliant prospect before them; but with Parry the enjoyment was heightened by the full realization of his hopes in this part of his voyage, and by the reflection that the serious responsibility he had incurred before leaving England would now redound to his honour.

Our limited space will not permit us to dwell upon the eventful progress of this, one of the most memorable of the Polar voyages. Suffice it to observe, that it was upon this occasion our navigators discovered the great opening into the Polar sea on the west, Prince Regent's Inlet on the south, Wellington Channel on the north, celebrated in after years as the supposed route of the gallant and

unfortunate Franklin, and farther west the islands known as the Parry Group, beyond which no subsequent expedition, with all the modern improvements and appliances of steam, has been able to proceed; and lastly, they discovered Banks's Land in the south-west, memorable as the furthest point afterwards reached by M'Clure from the opposite direction; an achievement which rendered certain the existence of the long-sought north-west passage. Between this land and the Parry Group there was stretched an impenetrable barrier of ice, which from that time to the present has baffled every effort of our ships, and is the only small tract remaining to be navigated to render evident the practicability of the passage.

But although the endeavours of Parry were not crowned with success, as regarded the main object of the Expedition, yet it will always remain as a bright feature in his distinguished career, that he achieved the discovery of those two remarkable terminating points—the "*ultima Thule*" of the navigation both from the *east* and from the *west*, which no ship from either quarter has yet been able to pass.

Parry in his route towards this terminating point of his discoveries had passed the meridian of 110° west, and the Expedition became entitled to the reward of £5000, which had been offered by the Government as an encouragement to Arctic enterprize.

After an anxious and unavailing suspense in the hope of a favourable change in the ice, Parry put into port, to pass the first dreary winter ever encountered by a Government expedition in so high a latitude: and here the qualities of Parry, which among others so peculiarly fitted him for the conduct of such an undertaking, were displayed in a remarkable manner, in the arrangements of the ship and the establishment of those wholesome regulations for the health and comfort of the crew, and for the occupation of the mind, which he knew so well to be essential to the bodily vigour of the seaman, and to the prevention of that fatal disease the scurvy, which had almost invariably attended previous attempts to brave a winter in the Arctic regions.

Aware of the influence of personal example, he took an active part in the theatrical entertainments which were got up for the diversion of the crew, and being an excellent actor, he contributed in no small degree to their success. On the other hand, he was inde-

fatigable in all astronomical observations, which were carried on night after night upon the snow, with the thermometer frequently 30° below zero, when it was necessary to keep the chronometer in hot sand to prevent its stopping, and to case the telescope in soft leather to prevent its destroying the skin of the face of the observer. We mention these facts once for all, as they serve to illustrate the zeal and determination which marked his character, and how by force of example he stimulated those who had the happiness to serve under his command. In this case the effect was the establishment of the geographical position of his winter quarters with a degree of accuracy probably never attained by any Expedition in the same time, even in a milder latitude; the lunar observations alone amounting to nearly 10,000.

As the spring advanced he conducted an overland journey across Melville Island, and discovered the sea on the north and Liddon Gulf on the west, where he left his broken cart, which served to mark indisputably his position to M'Clintock, who visited the spot thirty years afterwards and found the precious relic.

The summer of 1820 had well-nigh passed away before there was any possibility of liberating the ships from their winter quarters, and there being no prospect of a change in the great barrier of ice which covered the sea in every direction, and which indeed had never varied, Parry determined upon returning to England, where he arrived in October 1820.

As might be expected, his reception by his country was enthusiastic and most gratifying to him. He was immediately promoted to the rank of Commander. Bath, his native place, presented him with the freedom of its city; the Bedfordean gold medal was unanimously voted to him with a sum of 500 guineas; and he was presented with a silver vase, bearing ornamental devices emblematical of the Polar regions.

In December of the same year it was determined to follow up Arctic discovery by another Expedition, the command of which was again given to Parry. The attempt on this occasion was to be made by way of Hudson's Strait and Sir Thomas Rowe's Welcome. The Expedition left England in May 1821, and succeeded in making important additions to the geography of the Arctic seas, in clearing up various doubts respecting the statements

of early navigators, and in the discovery of a strait leading from Sir Thomas Rowe's Welcome into Prince Regent's Inlet. But the undertaking failed in its main object. The strait, which was named after the 'Hecla' and 'Fury,' and which proved to be the only outlet, was found to be impassable, and Parry, after passing two winters and encountering frequent and imminent perils from the rapid tides and whirling masses of ice which beset the ships and irresistibly carried them away from their positions, returned to England. Parry's narrative of this Expedition is one of the most interesting of the Polar voyages, from the long and intimate intercourse which was held with the Esquimaux tribes, and the exquisite embellishments from the pencil of his colleague Captain Lyon.

Immediately on the return of the Expedition, the Admiralty marked the high estimation in which they held the services of its commander by promoting him to the rank of Captain, and appointing him Acting Hydrographer, and the City of Winchester honoured him with its freedom.

A year of repose had scarcely elapsed when Captain Parry was summoned to take command of another Expedition destined to renew the attempt to reach Behring's Strait by way of Prince Regent's Inlet in connexion with an overland expedition under Captain Franklin. Parry left England in 1825 and succeeded only in reaching Port Bowen, where he passed his fourth dreary winter in the Arctic regions, and after experiencing great peril in the following summer from the ice and tides, which occasioned the loss of one of the ships and very nearly that of the other, he returned home.

This was the last of the Expeditions in a north-western direction under Captain Parry. The great energy and perseverance which had been displayed by him on all these occasions left no doubt in the minds of the Admiralty that further efforts in the same direction were likely to be fruitless, and for a while Arctic exploration had a respite. In these memorable voyages, under the command of the subject of our memoir, large acquisitions had been made to the geography of the Polar seas, and science had been promoted by numerous observations of a highly interesting and important character, some of which formed the subject of papers in the Transactions of this Society, by Captain Parry and his distinguished associates, Colonel Sabine and Lieutenant H. Forster.

It was seen, that, under able management and proper discipline, a winter in the Arctic regions could be passed, not only without those dreadful ravages which characterized the early voyages to those seas, but with as little if not less than the average mortality of mankind in civilized countries.

Captain Parry was now confirmed in his office as Hydrographer, and he was honoured by the freedom of the Borough of Lynn. In the autumn of this year Parry determined, since no passage could be found in a north-western direction, to propose to the Admiralty to renew the attempt to reach a high northern latitude by travelling over the vast expanse of ice which occupied the Spitzbergen seas. As early as 1818 a plan for effecting this object by means of light boats drawn by dogs had been submitted to the Admiralty by the late Sir John Franklin and his associate in the 'Trent,' Lieutenant, now Admiral, Beechey, and Parry now undertook to carry it into effect by means of rein-deer. He accordingly sailed in 1827 for Hammerfest, and taking on board a sufficient number of these animals proceeded to Spitzbergen, where he quitted his ship and commenced his perilous journey.

Those persons only who have seen the Spitzbergen ice and are acquainted with its rugged surface and the deep pools of water in its hollows, can judge of the enormous labour and difficulty in travelling over it. Yet Parry overcame these difficulties, and had it not been that the ice at length was found to have a motion to the southward nearly as fast as his party could advance to the northward, he would certainly have accomplished his object. All his efforts, however, were frustrated by this unforeseen circumstance, and after travelling 660 miles, a distance more than sufficient to reach the Pole in a direct line from where he set out, he found himself compelled to return. With what reluctance he submitted to this may be gathered from his journal, in which he observes, "dreary and cheerless as were the scenes we were about to leave, we never turned homewards with so little satisfaction as on this occasion." His furthest point reached on this journey was 82° 45′ N., a parallel which far exceeded any well-authenticated advance which had ever been made before. Had he been able to accomplish fifteen miles more he would have been entitled to the reward of one thousand pounds offered by the Government so long back as the days of Phipps.

On his return to England he resumed his duties as Hydrographer at the Admiralty, where he continued until 1829, when he accepted the office of Commissioner for the management of the affairs of the Australian Company. Before quitting England the King was pleased to mark his approbation of his services by conferring upon him the honour of Knighthood. The University of Oxford bestowed upon him the degree of D.C.L., he was elected a Fellow of this Society, and made an Honorary Member of the St. Petersburg Academy of Sciences, and also a Member of the Royal Irish Academy.

On his arrival in Australia he took up his residence at Port Stephen, a beautiful little bay about sixty miles to the north of Sydney. The quiet repose of this delightful spot was quite in unison with the mind of Parry, and while zealously discharging his duties as Commissioner of the Colony, he managed to promote among the colonists by whom he was surrounded, a spirit of piety and devotion to which they had before been strangers. He built a church, in which, in the absence of any authorized clergyman, he officiated himself.

After a residence of five years in Australia he returned to England, and accepted the office of Poor Law Commissioner in the county of Norfolk. The duties of this appointment were, however, by no means congenial to Parry. He was frequently called upon to adjudicate in cases in which his judgment was at variance with his feelings, and in about a year he resigned his appointment.

About this time he was selected to organize and conduct a newly-created department of the public service with the title of Comptroller of Steam Machinery, and continued to discharge the duties of this office with zeal and ability for ten years. During that time he saw introduced into the Royal Navy the screw-propeller, now so justly regarded as indispensable in our fleets, and did much to extend and improve the steam power of this country.

In 1847, finding his health begin to suffer from the onerous duties of his office, he accepted the appointment of Captain Superintendent of Haslar Hospital, which he held until his promotion to his Flag.

In 1853, the Lieutenant-Governorship of Greenwich Hospital falling vacant, it was offered to Sir Edward Parry, who accepted it, greatly to the satisfaction of his friends, who rejoiced in the expec-

tation that the quiet duties of that appointment would be beneficial to his general health, for they could not fail to notice the inroads which a life of so laborious and anxious a character had made upon his constitution. Scarcely, however, had six months elapsed before symptoms of a dangerous and painful disease became apparent. The probable fatal tendency of this complaint was well known to Parry, but he bore up against its painful effects with christian fortitude, cheerfulness and resignation.

Towards 1855 the approaching fatal termination of his complaint became but too evident, and at the recommendation of his medical advisers he determined to try the waters of Ems, but his bodily strength was unequal to the journey. He was detained for a time at Coblentz by exhaustion; and reached Ems, only to end there his days, for on the 8th of July, 1855, it pleased the Almighty disposer of events to bring to a close his long and varied life of usefulness. Perfectly resigned and full of humble hope he breathed his last, surrounded by his mourning family; giving proof in the closing moments of his existence of the blessed effects of that spirit of piety and devotion which he had so ardently cultivated throughout his life. His remains were conveyed to England and interred with honours in Greenwich Hospital.

Thus terminated the career of one of the most distinguished officers of the age in which he lived, a career as varied and eventful as it was honourable and prosperous: gifted with talents of a general character, he performed with credit whatever he undertook; but in none of his appointments was he more successful than in that of commander of those expeditions of discovery which have so much contributed to his own fame and the honour of his country.

As a member of society, no one stood higher in general estimation: kind, affectionate, of high moral and religious principles, charitable and humane, his memory will long be cherished in quarters where good men most desire to be remembered, more particularly in the wards of Haslar Hospital, and in those useful charitable institutions known as the Sailor's Homes, asylums for the humble members of his profession, for whose temporal and spiritual improvement he had always so strenuously laboured.

Parry left several works behind him. Besides the narratives of his voyages, we find his name associated with three papers in the

Transactions of this Society. A small volume on 'Astronomy by Night,' published early in life; a volume on the 'Parental Character of God,' now undergoing a fifth edition; and an 'Address to the Sailor.' His correspondence with the Admiralty, who consulted him upon all matters connected with the Arctic seas and the search for Sir John Franklin, is voluminous, and with his journals and observations have always been considered most valuable; and his voyages will long keep their places by the side of those of Cook, Anson, Vancouver, and other great navigators. The high estimation in which Parry was held by the Admiralty is marked by these frequent appeals to his opinion, as well as by all his appointments, and by a good-service pension being bestowed upon him; and lastly, by their reply to the Governor of Greenwich Hospital on being informed of his death, viz. that it was with deep regret they learnt that Her Majesty's Service had been deprived of so distinguished an ornament as Sir Edward Parry.

Sir Edward Parry married in 1827 Isabella Louisa, the fourth daughter of the late Lord Stanley of Alderley, who died in May 1839; and secondly, Catherine Edwards, daughter of the Rev. R. E. Hawkinson, and widow of Samuel Hoare, Esq., and left several children.

Richard Sheepshanks was born at Leeds, July 30, 1794. His father was engaged in the cloth manufacture, and destined his son to the same pursuit. At the age of fifteen, however, and after an ordinary school education, the son discovered his own preference for a learned profession, and the father accordingly placed him under the care of James Tate, head of the school at Richmond in Yorkshire, well known as one of the most successful teachers of his day. Here he remained until 1812, when he was removed to Trinity College, Cambridge. He took his degree with honours in 1816, obtained a fellowship in the next year, and proceeded to study for the bar, to which he was called about 1822. A weakness of sight, to which he was always subject, is supposed to have been the principal cause of his not practising law: but it must be added that his share of his father's property placed him in easy circumstances, independently of his fellowship, and his taste for science had become very decided. He took orders about 1824, and soon

began to devote himself entirely to astronomy. He became a Fellow of the Astronomical Society in 1834, and of this Society in 1830. Though often actively engaged in our behalf, and serving on the Council in 1832, his pursuits led him towards the Astronomical Society, of which he was always one of the most active of the executive body. His leisure, and his desire to help the young astronomer so long as he wanted advice and guidance, gave a peculiar value to his services, and a peculiar utility to his career.

Mr. Sheepshanks resided in London till about 1842, when he removed to Reading, where he died of apoplexy, August 4, 1855. There is much reason to suppose that his life was shortened by his laborious exertions in the restoration of the standard scale.

Though an ardent politician of the school of opinion which had to struggle for existence during the first half of his life, but gradually became victorious in the second, he never took any public part in a political question, except that of the Reform Bill. He was one of the Boundary Commissioners appointed in 1831 to fix the boundaries of boroughs under the new system of representation. His reading in politics and history was extensive, especially in military matters, with which he was very well acquainted; both ancient and modern tactics, from the best sources, having formed a portion, and no inconsiderable portion, of his studies. To this must be added literature and poetry, to which he was much attached: he never abandoned classical reading, and those who knew him best were often surprised at the extent to which he had cultivated modern literature.

But his subject was astronomy, and his especial part of that subject was the *astronomical instrument*. His reputation among astronomers on this point, and the articles which he contributed to the 'Penny Cyclopædia,' may allow us to regret that he did not draw up a full treatise on a matter which he had so completely fathomed.

Mr. Sheepshanks was engaged in active efforts on several special occasions, to which we make brief allusion. In 1828 he joined Mr. Airy in the pendulum operations in Cornwall, and suggested some of the most important plans of operation. In 1828 and 1829 he was active in the establishment of the Cambridge Observatory. In 1832 he was consulted on the part of the Admiralty, with reference to the edition then preparing of Groombridge's Circumpolar

Catalogue: the result was the appearance of that work in a much more efficient and more creditable form than it would otherwise have appeared in. In 1832 he also interfered in a matter to which, connected as it is with personal differences, we can only here allude, as eliciting much information on the subject of equatorial instruments in general; a result which is entirely due to the part taken by Mr. Sheepshanks. In 1838 he was engaged in the chronometric determination of the longitudes of Antwerp and Brussels: in 1844 in that of Valentia, Kingstown, and Liverpool. In 1843 and 1844, the subject of the Liverpool Observatory led him into a controversy, his pamphlets on which will be useful study to those who are interested in astronomical instruments. He was always an active member of the Board of Visitors at the Royal Observatory at Greenwich.

Mr. Sheepshanks was a member of both the Commissions (of 1838 and 1843) for the restoration of the standards of measure and weight, destroyed by fire in 1834. The standard of measure was placed in the hands of Francis Baily, at whose death Mr. Sheepshanks volunteered (Nov. 30, 1844) to continue the restoration. This matter occupied him closely during the last eleven years of his life. It would not be possible for us to give any detailed account of the operation, a full history of which is to come from the Astronomer Royal. We need only say, that after a thorough examination of the process, beginning with the very construction of thermometers,—a point which gave no small trouble,—results were obtained which were embodied in a bill which received the royal assent on the day following that on which Mr. Sheepshanks was struck by the shock which ended his life. The number of *recorded* micrometer observations is just five hundred short of ninety thousand.

Mr. Sheepshanks was especially distinguished by the integrity of his mind, and by his utter renunciation of self in all his pursuits. He did not court fame: it was enough for him that there was a useful object which could be advanced by the help of his time, his thought, and his purse. His consideration for others was made manifest by his active kindness to those with whom he was engaged, and no less by his ready appreciation of the merits of those against whom he had to contend in defence of truth and justice, as they

appeared to his mind. Nor must we omit to add, while using a qualifying expression to save the right of free opinion, and to avoid implying a decision which is not within our province, that in every one of his controversies, that which was truth and justice to the mind of Mr. Sheepshanks was nothing less to the minds of very many from whom no thinking man would differ without cautious examination. He was a devoted friend and a formidable opponent.

On the motion of J. P Gassiot, Esq., seconded by the Rev. Dr. Booth, the thanks of the Society were given to the President for his excellent address, and his Lordship was requested to permit the same to be printed.

Dr. Roget and William Spence, Esq. having, with the consent of the Society, been nominated Scrutators, the votes of the Fellows present were collected.

The following Noblemen and Gentleman were reported duly elected Officers and Council for the ensuing year:—

President—The Lord Wrottesley, M.A.

Treasurer—Colonel Edward Sabine, R.A.

Secretaries— William Sharpey, M.D.
George Gabriel Stokes, Esq., M.A.

Foreign Secretary—Rear-Admiral W. H. Smyth.

Other Members of the Council—The Duke of Argyll; Neil Arnott, M.D.; Rear-Admiral F. W. Beechey; Sir Benjamin Brodie, Bart.; William Benjamin Carpenter, M.D.; Arthur Cayley, Esq.; Rev. James Challis, M.A.; Charles Darwin, Esq., M.A.; Sir Philip de M. Grey Egerton, Bart.; William Fairbairn, Esq.; John Miers, Esq.; William Allen Miller, M.D.; William Hallows Miller, Esq., M.A.; James Paget, Esq.; John Stenhouse, LL.D.; Rev. Robert Walker.

The following table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Honorary.	Foreign.	Having compounded.	Paying £2 12s. Annually.	Paying £4 Annually.	Total.
December 1, 1854..	10	47	403	14	271	745
Since elected......	......	+4	+6	......	+11	+21
Re-admitted	......	......	......	......	+1	+ 1
Since compounded	......	......	+1	......	−1	
Defaulters	......	......	......	......	−2	− 2
Withdrawn	......	......	......	......	−1	− 1
Since deceased	−1	−2	−25	−1	−6	−35
November 30, 1855	9	49	385	13	273	729

Statement of the Receipts and Payments of the Royal Society between December 1, 1854, and November 30, 1855.

Receipts	£	s.	d.
Balance from last year in the hands of the Treasurer	1043	19	9
Subscriptions and Compositions	1795	16	0
Rents	186	16	9
Dividends on Stock	1063	8	6
Sale of Transactions and Proceedings	185	14	9
Balance due to the Treasurer	255	9	8
	£4531	5	5

Estates and Property of the Royal Society, including Trust Fund.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £116 16*s.*

Estate at Acton, Middlesex (29 A. 0 R. 1 P.), £60 0*s.* 0*d.*

Fee farm rent in Sussex, £19 4*s.* per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.

£14,000 Reduced 3 per cent. Annuities.

£24,479 4*s.* 7*d.* Consolidated Bank Annuities.

£513 9*s.* 8*d.* New 2½ per cent. Stock.

EDWARD SABINE,

Treasurer.

Payments	£	s.	d.
Dr. Tyndall, Bakerian Lecture	4	0	0
Salaries	728	0	0
Purchase of £2172 13*s.* 2*d.* 3 per cent. Consols	2000	0	0
Fire Insurance	45	1	6
Printing Transactions	213	14	3
Ditto Proceedings	303	16	6
Ditto Miscellaneous	62	1	0
Engraving	231	19	9
Paper for Transactions and Proceedings	166	7	6
Binding Transactions	47	4	2
Books Purchased and Binding	323	3	0
Stationery	15	16	3
Shipping Expenses	13	10	7
Fire and Lighting	81	0	0
House Expenses	125	4	6
Taxes	52	7	6
Wintringham Fund	34	8	6
Miscellaneous and Petty Charges	83	10	5
	£4531	5	5

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END OF THE SEVENTH VOLUME.

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